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Double income households

4 essays on children, votes and parents' labor supply

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Preface

This thesis was written as part of my research fellowship at the Department of Economics and ESOP (Center for Studies of Equality, Social Organization and Performance) at the University of Oslo. I thank the department and ESOP for giving me an inspiring and good professional environment to learn and do research.

I thank my supervisors Jo Thori Lind, Kalle Moene, and in a shorter period Oddbjørn Raaum. Their advice has been constructive and inspiring and has greatly improved the quality of my work. A special thanks to my coauthor Sara Cools - working with her has made me learn tremendously much. Thanks also to my coauthor Simen Markussen for inspiring cooperation in the last year.

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Chapter 1

Introduction

The household forms an important decision and production unit in the economy. To what extent household characteristics are important for individual choices and economic outcomes is the overall question of this thesis. The most weight is given to household labor supply decisions in the presence of children - and how children affect the further careers of husband and wife, measured by wages. I also study how the relative economic interests of husband and wife are represented in the political system through the effect of relative income on individual voting behavior.

The first chapter studies how men and women's labor supply depend on own and spouse wage, how labor supply changes with children - and how the importance of relative wages in the household changes with children. The second chapter studies how having children affects wages - and thereby the role of children in changing the relative wages of men and women. The third chapter addresses the endogeneity of the child-decision and estimates the impact of an exogenous distribution of children (using miscarriage as a proxy) on fertility and labor market outcomes for women. The fourth chapter studies which income in the household best predicts individual voting behavior - own income or spouse income.

Chapter 2: Children, labor supply and wage elasticities

Having children causes a radical change in the household's need for home production. It alters the degree of substitutability between home and market hours - and the substitutability between spouses' hours at home and in the market.

Mincer (1962) was the first to study female labor force participation with the household as the appropriate decision unit, recognizing that women substitute market hours for home production, not just leisure. With men increasingly taking share in the caring for children, having children also potentially changes the substitutability of men's market hours to home hours. In addition, having children may increase the substitutability of spouses' market hours. With the basic insight of Becker (1981), spouses can specialize in different tasks and thereby increase the gains to marriage.

In this paper we investigate the respective roles of children and relative wages in determining household specialization on Norwegian administrative data and wage statistics from 1997-2007. More specifically, we estimate the effect of having children on men and women's working hours, as well as the effect of wages on working hours - both their own and their spouse's (known as *own* and *cross wage elasticities*). In addition, we estimate how having children affects the own and cross wage elasticities, by including the interaction between wages and children. This has not been estimated before in the empirical literature, and here lies the main contribution of this paper.

Our results show that the presence of children has the largest average effects for female labor supply; both for labor force participation and for working hours. The increase in home production also raises women's responsiveness to wage changes; the own wage elasticities become more positive while the cross wage elasticities become more negative. This is consistent with the substitution effect between home and market hours becoming stronger with children. For men, the presence of children has less impact both on the levels and on the wage responsiveness of labor force participation and working hours. There is however an average negative effect on men's working hours, and their cross wage elasticity is more negative after children. This indicates that the presence of children increases the substitutability of spouses' market work in the household, and underlines the importance of children also for men's labor supply.

Chapter 3: Parenthood wage penalties in a double income society

Having children affects labor supply negatively, as we saw in the previous chapter. The negative effect on labor supply may in turn lead to lower wages, for instance through the effect it has on human capital (Mincer and Polachek, 1974). Internationally, there is a large literature documenting a negative association between having children and women's wages, and a smaller literature documenting a positive association between having children and wages for men. The Norwegian context is different in many respects, with high female labor force participation, one of the narrowest gender wage gaps in the OECD countries, and a generous welfare system with various policies securing child care, paid parental leave and job protection. We show that in this context, the motherhood penalties are still significant, but we also find that men experience a negative private cost from fatherhood.

We use data from official Norwegian registries on wages and income, covering about 70% of the working population in the years from 1997 to 2007.Using individual fixed effects estimation on a sample of individuals who are observed to have at least one child by 2008, we find a substantial wage penalty for women - ranging from 1.7% for women with lower secondary education to 4.7% for women with higher education, higher degree. Contrary to most other studies, we find negative (though comparatively small) effects of having children on men's wages - around 0.4% to 0.5%. This is consistent with an increasing role of fathers as care-givers.

The estimated penalty for women is only partly explained when we include measures of experience, parental leave, working part time and sector of employment. For men on the other hand, variation in parental leave explains half of the fatherhood wage penalty. We find the largest penalties to parenthood in the private sector, for full time workers, and for those who take the longest leave.

Chapter 4: Random kids - causal inferences from using miscarriage as a natural experiment

We saw in the previous chapters that having children has large impacts on labor market outcomes. Children are not exogenous to labor market outcomes, however. Whether to have children and when to have them are decisions the household make, and may be influenced by economic circumstances, like career opportunities. Having children is therefore influenced by labor market outcomes - which in turn are influenced by having children. The two directions of causality is also reflected in different research traditions; labor economists treat children as an independent variable influencing labor market outcomes, while demographers treat labor market outcomes as the independent variable influencing fertility choices.

Miscarriage randomly prevents the birth of a child. It thus provides unique variation in whether a woman has a child at the planned point in time which we exploit to estimate the causal impact of an exogenous distribution of children on labor market outcomes. The potential impact of economic factors on who becomes parents and at what time is equal for the group that miscarries and the group that gives birth. The only difference between the groups is that some have children while some do not in the planned birth-year. This will ensure the causal interpretation of the effect going from having children to labor market outcomes.

We estimate the effect of miscarriage on five different family outcomes; whether an individual has children at all, timing of birth, number of children, spacing of siblings and age of youngest child. Further, we estimate the effect of miscarriage on four different labor market outcomes; earnings, labor market participation, weekly hours and hourly wages. We interpret the effect of miscarriage on labor market outcomes as the effect of randomly distributing children for women who plan to have children at the same time.

Having a miscarriage has what seems like permanent consequences for fertility outcomes. 1 out of 5 having a miscarriage still has no children five years later. The number for those who miscarry at second birth are approximately the same. Furthermore, those who miscarry also have fewer children and for those that have more than one child, the spacing between their children is shorter than for others. Despite permanent consequences for fertility it seems to be very few such long term consequences for labor-market outcomes. Whereas employment and sickness absence are dramatically affected during pregnancies and the first one or two years after birth, those who miscarry and those who give birth are almost identical 5 years later, regardless of whether we compare wages, earnings, employment, sickness absence or social insurance dependency.

Chapter 5: How husbands and wives vote

In this chapter, I study to what extent the relative incomes in a household is represented in the political system through the effect that the relative incomes has on individual voting behavior.

In basic models in political economy, like the Median-voter model, individual political preferences are a function of individual incomes (Meltzer and Richard, 1981). The basic insight is that redistribution is less beneficial for high-income earners who therefore demand lower levels of redistribution. These models abstract from the fact that most individuals are part of a household, and that this household will influence your economic position and/or your political views. Most major surveys on voting behavior (like the World Values Survey, the Eurobarometer, American National Election Studies and a number of other election studies) take the other extreme and only ask for household income.

I use the National Child Development Study (NCDS) and the British Cohort Study (BCS) which are detailed survey data on two British cohorts born in 1958 and 1970 to study the relative importance of own versus spouse income in determining political preferences. The data are unique for my purpose, as it is the only data to my knowledge that contain both individual income and spouse income in combination with individual voting behavior. In addition, the data have a panel structure, which enables me to investigate the role of income over the life-cycle. The empirical method I use is OLS regression to estimate the quantitative importance of own versus household income.

I show that predictions of voting behavior based on individual income and household income give very different results, especially for women who are not the main earners of the family. I find that individual income is only important for women if their income is fairly representative of the household - if they work full-time or earn a higher income than their husband. Otherwise, their husband's income has a much larger impact. Men always vote according to individual income. Even in the cases where he earns less than his wife, his wife's income has no significant impact on his voting behavior. On average, household income is therefore the best predictor of both men and women's voting behavior, but this is mainly the result of women's average economic position in the family.

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Chapter 2

Children, labor supply and wage elasticities

Sara Cools[1] Marte Strøm[2]

Abstract Having children causes a radical change in the household's need for home production. It alters the degree of substitutability between home and market hours - and the substitutability between spouses' hours at home and in the market. We find that, conditional on being employed, the level of both men and women's weekly working hours is reduced after having children; women's by 12% and men's by 1.5%. Women's labor force participation is reduced by 10% while men's participation increases with 0.5%. We also find that having children increases the substitutability of market and home hours, as reflected in a more positive own wage elasticity, and the substitutability between spouse's market work, as reflected in a more negative cross wage elasticity. The change in the own wage elasticity is more marked for women than for men, in line with the assumption that home and market hours are more of a substitute for women. The marked change in both men and women's cross wage elasticity shows, however, that spouses' hours are substitutes to a larger extent after having children.

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2.1 Introduction

The causal relationship between having children, the parents' wages and household specialization is not clear cut. One the one hand, children increase the scope for specialization within the household, and wages form economic incentives influencing the decision on how to specialize. On the other hand, the degree of specialization will influence relative productivity both in the market, as is reflected in wages, and at home. As stated in Lundberg and Rose (1999), "[for] any couple, the initial gains to specialization will be reinforced over time as husband and wife acquire skills specific to the market or domestic sectors".

In this paper we investigate the respective roles of children and relative wages in determining household specialization. More specifically, we estimate the effect of having children on men and women's working hours, as well as the effect of wages on working hours - both their own and their spouse's (known as *own* and *cross wage elasticities*). In addition, we estimate how the partial response to either varies with the other, that is, how the response in working hours to having children varies with an individual's own wage rate and with that of their spouse. Conversely, we estimate how having children affects the own and cross wage elasticities.

The effect of the interaction between wages and children on labor supply has not been estimated before in the empirical literature, and here lies the main contribution of this paper. Having children constitutes probably the largest increase in home production in the household. If home production is a closer substitute to women's market work (for economic, social or cultural reasons), the substitution effect [of what? wage increases?] should be stronger for women after they have children. The extent to which men's wage elasticities change in the same way will reflect whether child care is also a close substitute to men's market work. The changes in wage elasticities for the husband and wife will in addition reflect to what extent household labor supply depend on the relative wages of the spouses after children.

The paper relates to two somewhat separate strands in the empirical literature on family and female labor force participation. First, there is a literature on individual wage elasticities within the household, estimating own and cross wage elasticities of men and women (Blundell et al., 1998; Blundell and MaCurdy, 1999; Devereux, 2004; Blau and Kahn, 2007). Although this tradition recognizes the importance of home production for (especially) female labor supply elasticities, children are often treated as exogenous and used as control variables. Secondly, there is a literature on how children affect labor supply in itself, taking into account the endogeneity of the childbearing decision (see Rosenzweig and Wolpin (1980) and Angrist and Evans (1998) for important contributions). In both strands of the literature, women's labor supply is found to be more responsive than men's, both to having children and to own and spouse's wage.

We use a panel on wage and working hours for Norwegian households for the years from 1997 to 2007. We rely on different estimation methods in order to deal with issues of endogeneity, both of having children and of wages, when estimating the determinants of individual labor supply.

To identify the wage elasticities we instrument individual wages, using average hourly wages in different groups based on gender, education, cohort and region of residence. Instrumenting wages in this way reduces measurement error and cuts the reverse causal link going *from* labor supply *to* hourly wage. To identify the change in behavior before and after the household has children, we exploit the panel structure of the data, including individual fixed effects to use the within-individual variation in labor supply.[3] In addition, we restrict the sample to households we observe to have children during the time window, in order to avoid problems of time-variant heterogeneity that affects both hours and the probability of having children.

Our results show that the presence of children has the largest average effects for female labor supply; both for labor force participation and for working hours. The increase in home production also increases women's responsiveness to wage changes; the own wage elasticities become more positive while the cross wage elasticities become more negative. This is consistent with the substitution effect between home and market hours becoming stronger with children. For men, the presence of children has less impact both on the levels and on the wage responsiveness of labor force participation and working hours. There is however an average negative effect on men's working hours, and their cross wage elasticity is more negative after children. This indicates that the presence of children increases the substitutability of spouses' market work in the household, and underlines the importance of children also for men's labor supply.

The rest of the paper is organized as follows: Section 3.2 briefly discusses the literature on children, female labor supply and household specialization. Section 2.3 presents our empirical strategy. In Section 3.4 we give a description of our data. In Section 2.5 we present the results of our estimations of the effect of children on labor supply and wage elasticities in the household. Section 2.6 summarizes the results, and Section 3.8 concludes.

2.2 The literature on children and household specialization

Jacob Mincer in his seminal paper was the first to study empirically female labor force participation with the household as the appropriate decision unit (Mincer, 1962). In the same paper he abandoned the usual theory of seeing workers as substituting only between market work and leisure. He recognized that non-market activity includes also household

[3] Labor supply is estimated separately for men and women, with individual fixed effects. The individual is however part of the same household over the whole time window and the individual fixed effect therefore captures fixed characteristics of the husband as well.

work and child care. Individual market wages and household productivities will determine the allocation of labor between the market and the non-market activities. He found larger own wage elasticities for women and explained this by the difference between husband and wife in non-market activities; household production is a more substitutable activity for market work than leisure.

The substitution of labor in the household between market and home production depending on the spouses' relative productivities at both activities is also the basis of the unitary model of household behavior (Becker, 1974, 1981)[4]. Since consumption is shared, it allows the household to make use of their comparative advantages at market and at home production.

Seeing women as substituting between market and home production in addition to leisure has proved an influential and fruitful way to analyze female labor supply behavior over time. Growth in female labor supply over time has been interpreted in light of the relative importance of income and substitution effects in different time periods (see e.g. Goldin (2006)). Blau and Kahn (2007) compare men and women's wage elasticities over the period 1980-2000 and find a strong decrease in female wage elasticities over the period. Their suggested reason is that the substitution effect is weaker with lower levels of home production. With an increasing trend that men and women share more in home production, they hypothesize that men and women's wage elasticities will continue converging.

In general, women's labor supply is found to be much more wage elastic than men's. For both men and women, own wage elasticities are higher than cross wage elasticities. Blundell and MaCurdy (1999) report a median own wage labor supply elasticity, based on 18-20 estimates, of 0.08 for married men and 0.78 for married women. Women's cross wage elasticity is also found to be much higher than men's (Kooreman and Kapteyn, 1986; Killingsworth, 1984; Devereux, 2004), but Blau and Kahn (2007) find that it declined over the last decades of the 20th century, thus approaching men's in size.

(Blau and Kahn, 2007) report higher elasticities for mothers than for non-mothers. Using a simultaneous equations approach, Lundberg (1988) finds no interaction in spouses' labor supply in households without children, but does find interaction in work hours and a negative cross earnings effect in households with children. Dalmia and Sicilian (2008) find positive assortative matching on age, education, income and hours worked for couples without children, but negative assortative matching on income and hours worked in older marriages and marriages with children. They interpret this as a sign of specialization when a couple has children. However, as these analyses are cross sectional, the larger wage elasticities in families with children may at least partly be due to selection of families with more interrelated labor supply into parenthood.

[4]Chiappori (1988) has the most general representation of household decision making, only imposing Pareto efficiency

There is to our knowledge no study of the change in elasticities within families moving from being a couple to having children. If wage elasticities are different at different stages in the life cycle, this is in accordance with the insights of Heckman and Macurdy (1980) on the non-substitutability of non-market time at two different ages (a result that did not find support in their empirical analysis).

There is an extensive literature documenting a negative effect on women's labor supply of having children. The earlier literature is reviewed in Browning (1992), later literature includes for instance Angrist and Evans (1998); Lundberg and Rose (2000). There is some evidence of a positive effect on men's labor supply (Pencavel, 1986; Lundberg and Rose, 2002; Simonsen and Skipper, 2008). Together, they indicate that some degree of specialization takes place within the average household.

Some studies on the labor supply effect of children link spouses' labor supply, among others Angrist and Evans (1998); Lundberg and Rose (2002). Angrist and Evans (1998) look at the effect on men and women's labor supply by educational level of the wife, but find no significant heterogeneity. Lundberg and Rose (2002) look at the effect of children on men's hours if the wife is continuously employed or not, and find his hours are positively affected if she has a career break while his hours are negatively affected if the wife is continuously employed. These studies do not however include wages as explanatory variables in their model.

2.3 Estimation

In this paper, we both want to estimate the effect of children on labor supply, own and cross wage elasticities and the interaction between the two. Both wages and having children are potentially endogenous regressors.

To identify the effect of having children on household labor supply, we avoid the problem of selection into parenthood by restricting our sample to those couples who are observed to have a child together.[5] We also include individual fixed effects in some specifications to control for time-invariant heterogeneity that both influences the level of hours and the timing of children.

To identify the wage elasticities, we instrument wages with the mean wage of individuals within the same educational group, cohort, gender, region and year. By doing this, we exploit the changes in an individual's wage that is unrelated to the individual's own labor supply history or unobserved characteristics.

The different methods are discussed more thoroughly below.

[5] Cools and Strøm (2012) show that men who become fathers are on a steeper wage path than men who do not become fathers.

2.3.1 Empirical specification

Our reduced form model of an individual's labor supply is

$$\begin{aligned} LS_{it} = {} & \beta \ln w_{it} + \beta_s \ln w^s_{it} + \beta_I \ln I_{it} + \pi Child \\ & + \rho Child \times \ln w_{it} + \rho_s Child \times \ln w^s_{it} + \rho_I Child \times \ln I_{it} \\ & + \gamma X_{it} + \eta_i + \nu_t + \epsilon_{it} \end{aligned} \tag{1}$$

We estimate the model using linear estimation methods. LS_{it} is the measure of individual i's labor supply in year t - either the natural logarithm of weekly working hours or a dummy indicating labor force participation. w is the individual's hourly wage, w^s is the hourly wage of the spouse and I is the household's capital income. β is our estimate of own wage elasticity before children, β_s our estimate of cross wage elasticity before children.

$Child$ is an indicator variable for whether the couple has had their first child, and π is our estimated coefficient of the average effect of having children. Our estimated change in own and cross wage elasticities after children is therefore ρ and ρ_s respectively.

X is a vector of age dummies (each spanning three years) for both parents, and indicator variables for expecting a first child or having a first child younger than one year. We control for being pregnant with and having a baby because we are interested in the change in hours and wage elasticities from normal labor market behavior before the couple becomes parents to the labor market behavior after the child has turned one year. [6]

η are individual fixed effects, and ν are year fixed effects. ϵ is the error term, clustered at the individual level.[7]

2.3.2 Constructing households

Linking the parents of a child to each other is crucial for the analysis in this paper. Only marital status can be observed over the whole time window that we use, cohabitation status is available only from 2002. In Norway, there are equally many children born to cohabiting parents as to married parents.[8]

[6] We wish to weed out unusual labor market behavior during these periods. Only to mention some: Women are more sick during pregnancy, and the Norwegian parental leave system provides incentives for both parents to seek employment when expecting a child. During the first year after birth, one parent can stay at home with full wage compensation - and some kindergartens only accept children that are one year or older.

[7] Ideally, standard errors would be clustered at the IV group of both spouses, in addition to at the individual/household level. This is not feasible.

[8] In 2007 the share born to married parents was 45%. 11% were born to single mothers, and the remaining 44% to cohabiting parents. The total number of live births was 58459. Source: `http://statbank.ssb.no/statistikkbanken/`

Because we do not want to limit our sample to married couples only, we use the linking of children to their parents in order to identify what we will call “spouses” in this paper: They are parents who have their first child together. In addition, neither parent is registered as being a single parent the year the child is born, and neither parent already has a child from a previous relationship.

We use observations on these parents from four years before their first child is born until the first child is four years old. The exact choice of four years is the result of the trade-off between having many observations and the probability of capturing a couple who is actually living together, both before and after the child is born.

This way of constructing households matters for the external validity of our estimated wage elasticities for couples without children. Our estimates may be understood as estimates of wage elasticities in the period just before having children.

2.3.3 Individual fixed effects

Since we have panel data, we have the possibility to correct for time-invariant heterogeneity by including individual fixed effects in the estimations. Since the individuals in our sample by construction belong to the same household over the whole time window, individual fixed effects will also capture fixed effects of the spouse and household. Estimating the relationship in Equation 1 may suffer from omitted variables bias if there are unobserved characteristics of the household that both determine hours, when to have children and the mean wage of the IV group that the individual belongs to. Examples of omitted variables that would be of particular importance here are tastes for work and views on gender roles. If these unobserved characteristics are time-invariant, we control for them by including individual fixed effects.

There are still potential problems with time-*variant* heterogeneity. If unobserved characteristics change over the life cycle and the change is correlated with changes in labor supply and with changes in wages (or with changes in the mean wage of the IV group when we use instrumental variables), we cannot fully control for this. An example is that if households with more diverging wages (measured by their own wage or by their IV group wage) change their views on gender roles more than households with less divergence in wages, the resulting labor sharing in the household will seem dependent on the relative wages when it is really dependent on the views on gender roles.

Including individual fixed effects means that we only use the variation within the different observations for the same individual. The wage elasticities will be identified by those individuals who change the relevant labor supply behavior during the sample period. 49% of our the women and 40% of the men in our Wage statistic sample change working hours - and 21% of the women and 8% of the men in the labor force participation sample change participation status - at least once over the period.

Hourly wages are generally prone to measurement error, and our data are no exception. Measurement error creates an attenuation bias in the estimate, which is even larger in a within-estimator (Solon, 1985; Griliches and Hausman, 1986). By instrumenting wages, we also correct for measurement error.

2.3.4 Instrumenting wages

To accommodate the problem of reverse causality, omitted variable bias and measurement error when estimating the impact of wages on labor supply, we instrument wages in a given year using the mean wage that year of individuals of the same sex who have the same education, live in the same region and who are born within the same 3 year cohort. Our instrument is similar to those applied by Blundell et al. (1998), who use education/cohort groups, and Devereux (2004), who uses groups based on the interaction of husband's and wife's education/cohort, plus region.[9]

The women and men in our sample belong to 247 and 254 distinct education groups, respectively, based on the three first digits of the six-digit education code provided by Statistics Norway ("NUS2000"). The first digit indicates one out of nine levels of education, running from no education (defined as less than mandatory education in Norway) to training as researcher (20 years of education or more). The second digit indicates one out of nine broad fields of education ("fagfelt"), and the third digit further divides these fields into nine groups. Together, the second and third digit define narrow fields of education ("faggrupper"). In combination with the first digit, indicating the length of the education, we believe that this is the relevant level when instrumenting wages.[10]

Women are divided into 14 "birth cohorts", each spanning three years. As we have a less strict requirement on the age of men in our sample, they are sorted into 20 different cohorts. Finally, there are 46 different regions.

The instrumental variable is constructed by computing the mean wage within each such education/cohort/region group (in the following referred to as the *IV group*), subtracting the individual's own wage. Naturally, this computation is done using all individuals in the original data (Statistics Norway's "Wage statistic", described more in detail in Section 2.4.1) who have finished their education at the time of observation - we do not restrict the observations underlying the generation of the instrumental variable to those who are included in our final sample. In total, the women in our sample belong to 26245 different groups, and the men to 27104 different groups. In order to avoid small sample bias in our instruments, we exclude groups with less than 12 observations in the original data set from our analyses, and thus we end up with 15472 and 14555 different groups for women

[9] Blau and Kahn (2007) use income decile.

[10] A full description of Statistics Norway's education data can be found at `http://www.ssb.no/english/subjects/04/90/nos_c751_en/nos_c751_en.pdf`

and men respectively, where the median number of observations underlying the group means each year is 100 and 127 respectively.

The first stage underlying our IV 2SLS estimates is

$$\begin{aligned}\ln Z_{it} = &\hat{\beta} \ln \bar{w}_{it} + \hat{\beta}_s \ln \bar{w}^s_{it} + \hat{\beta}_I \ln I_{it} + \hat{\pi} Child \\ &+ \hat{\rho} Child \times \ln \bar{w}_{it} + \hat{\rho}_s Child \times \ln \bar{w}^s_{it} + \hat{\rho}_I Child \times \ln I_{it} \\ &+ \hat{\gamma} X_{it} + \mu_t + \zeta_i + \varepsilon_{it}\end{aligned} \tag{2}$$

Z_{it} is either one of our four instrumental variables for individual i in year t; the individual's own wage, their spouse's wage, own wage interacted with the dummy for having children, and spouse wage likewise interacted. $\bar{w}$ is the mean wage of the individual's IV group (own wage not included), $\bar{w}^s$ is the mean wage of the IV group of the spouse, I is household capital income, and $Child1$ is a dummy indicating the presence of a first child older than one year. X is a series of age dummies for the individual and the spouse, each group spanning 3 years (corresponding to the IV group cohorts). μ_t is a vector of year dummies. ζ_i is treated as a separate entity in specifications including individual fixed effects.

Table 1 reports the first stage results for our instrumental variables, each column shows the coefficients from a regression of Equation 2 for every instrumented variable. There are two panels, Panel A reports first stage result without including individual fixed effects, in Panel B individual fixed effects are included.

Along the diagonal of Panel A, we see that the instruments are very strong predictors of the corresponding instrumented variables, considerably stronger than the other instruments. The "right" instrument predicts the different wage variables. The coefficients in each column also sum to less than one, which is reassuring.

Panel B reports the first stage of the estimation with fixed effects. There are large and strongly significant coefficients along the diagonal also here. However, in columns (3) and (4), where the instrumented variables are the interactions of wages and the child dummy (therefore representing the change in wages after children), also wages before children are strong predictors, with an equally large, negative coefficient. This is due to the fact that the first period change after children will have the interaction going from zero to a strictly positive number, as the zero observation is negatively correlated to the group mean wage, as has no impact on the validity of the estimates in the second stage.

The exclusion restriction on our instrumental variables is that the mean wage in the individual's (or their spouse's) IV group is only related to hours worked by the individual through its relation to the individual's wage. This is admittedly a strong assumption to make. For instance, a larger demand for the competence of individuals in a particular education group might at the same time increase their wages *and* their hours. Our IV reduces the problem of reverse causality and measurement error. Time-invariant heterogeneity is

Table 1: First stage results

	Mother wage	Father wage	Mother wage × C1	Father wage × C1
Panel A: OLS				
Mean wage in mother's IV group	0.79***	0.15***	-0.04***	-0.02***
	(0.01)	(0.01)	(0.00)	(0.00)
Mean wage in father's IV group	0.10***	0.74***	-0.02***	0.00
	(0.01)	(0.01)	(0.00)	(0.00)
Mean wage in mother's IV group × C1	0.01	0.04***	0.86***	0.23***
	(0.01)	(0.01)	(0.01)	(0.01)
Mean wage in father's IV group × C1	-0.01	0.00	0.11***	0.74***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	176139	176141	176139	176141

	Mother wage	Father wage	Mother wage × C1	Father wage × C1
Panel B: FE				
Mean wage in mother's IV group	0.24***	0.08***	-0.27***	-0.04***
	(0.05)	(0.01)	(0.04)	(0.01)
Mean wage in father's IV group	0.08***	0.26***	-0.04***	-0.28***
	(0.01)	(0.01)	(0.01)	(0.02)
Mean wage in mother's IV group × C1	0.03**	0.01	0.84***	0.20***
	(0.01)	(0.01)	(0.01)	(0.01)
Mean wage in father's IV group × C1	-0.02***	0.05***	0.11***	0.76***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	176139	176141	176139	176141

Note: Each column provides estimates from a regression based on Equation 2, the outcome variable corresponding to the column header. Household capital income, a dummy for having a first child and the interaction between them are included in each regression. So are year dummies and age dummies for both spouses, plus a dummy for expecting a child and for having a baby younger than one year. In the lower panel individual fixed effects are included. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

controlled for by including household fixed effects. If there is time-variant heterogeneity that correlates both with the mean wage of the IV group and with individual working hours, we cannot fully control for this.

2.3.5 Alternative IV

The presence of children in the household is thoroughly documented to influence hourly wages. Because we wish to investigate the relative roles of spouses' wages, this poses a problem also when wages are instrumented as described above, as the incidence of children is likely to be correlated between individuals in the same IV group and the decisions made by household's on labor sharing is likely to be correlated with the labor sharing decisions in the households of the respective IV groups. That is, the instrument is not valid if unobserved household labor sharing is correlated both with the instrumental variables and with labor supply.

In order to accommodate this possibility, we use a modified IV as an alternative to the main construct described above. For this IV we only include wage observations for those individuals in the IV group who have not had children. We stick to the same requirement of there being at least 12 wage observations underlying the group mean for us to use it. For this alternative instrument we then end up with 11744 different IV groups for the women and 10781 different groups for the men in our sample, with respective median numbers of observations underlying the group means of 63 and 79.

The first stage results for these alternative instrumental variables are reported in Appendix Table 8.

2.3.6 Extrapolating wages

For our analysis of wage elasticities along the extensive margin (labor force participation), we must impute wages in years with missing observations, as wages are observed only when the individual is registered with employment and positive hours. Due to the sampling of our data (see Section 2.4.1), and because individuals who have breaks in the labor force participation are not systematically found in the lower end of the income distribution in Norway, we do not predict wages using the approach of Blau and Kahn (2007). Rather, we extrapolate and intrapolate wages linearly.

The wage measure used for the analysis of participation is instrumented in the same way as described in Section 2.3.4, the only difference is that where wages have been imputed the actual IV group mean wage is used, as there is no observation of the individual's own to be excluded. Appendix Table 9 and 10 give the first stage results for the instrumental variables used in the participation analysis.

2.4 Data and descriptive statistics

2.4.1 Outcome variables

Working hours

Our data on weekly working hours are constructed from the information on contracted working hours and overtime in Statistics Norway's "Wage statistic" ("Lønnsstatistikken"). The Wage statistic is based on employer reports for a sample of Norwegian enterprizes on all employees by the 1st of October. Every year all public enterprizes and all private enterprizes with more than a certain number[11] of employees are included, for the remaining private sector a 50% sample of medium size enterprizes and a 20% sample of small enterprizes is drawn every year.[12] On average, the Wage statistic covers about 80% of Norwegian employees (100% of the public sector employees and 70% of the private sector employees) every year.

Contracted hours are given either in numbers or in percentages.[13] Typically, public sector enterprizes report hours in percentages and private sector enterprizes report a number of hours per week. When reported in percentages, we use 100% = 37.5 hours per week. We sum contracted hours across all reported employment of the individual within each year.[14]

Overtime is reported in hours per month. We set negative overtime to zero and overtime is truncated at 100 hours per month. Overtime is then multiplied by $12 \times 7/365$ in order to get hours of overtime per week. Alas, it turns out that there are no reports of overtime in the years 2000, 2001, 2002 and 2005. Also, there are comparatively few reports of overtime in 1998 and 1999. There is little reason to suspect this reporting error to be systematically linked to wages or having children.

Finally, the variable we use, weekly working hours, is the sum of weekly contracted hours and weekly overtime. This measure is also set to zero if negative, and truncated at 100 hours per month. Due to measurement error in the overtime variable, we also run robustness checks using only contracted hours.

Labor force participation

Our measure of participation is a dummy variable constructed from information in Statistic Norway's "Income registry" ("Registerbasert inntektsfil"). The Income registry is

[11]The number varies with industry and year.

[12]Employment in agriculture, hunting and forestry is left out. So are enterprizes with 3 or less employees.

[13]We have replaced the contracted number of hours by one tenth of the original number if it exceeds 70 hours per week, as the distribution of hours above this threshold peaks at typical numbers of hours times ten (f.i., there are peaks at 150, 175, 350, 355 and 375 hours per week).

[14]We delete obvious duplicate reports.

based on tax reports and contains information on all types of income for every Norwegian resident.

If an individual's occupational income ("wyrkinnt"), i.e. the sum of wages and business income (income reported as wage income by the self-employed, not including capital income), exceeds 2 times the *basic amount* (G) of the Norwegian social security system, participation is set to one, zero otherwise.[15]

2.4.2 Explanatory variables

Hourly wages

The Wage statistic contains information on contracted monthly pay for every observation on contracted hours (see Section 2.4.1). For every employment observation we calculate hourly wages by dividing contracted monthly wages - multiplied by $12 \times 7/365$ - by contracted hours per week. For individual's who are registered with more than one employment in a given year, we choose as the hourly wage from the employment where the individual works the most contracted hours (and in case of a tie, where he or she gets the most contracted wages) to represent the hourly wage of the individual that year.

Capital income

The information on individual capital income comes from the Income registry (see Section 2.4.1), and is the sum of interests, dividends, realized profits net of realized loss and other capital income during each year. As households often share capital ownership, investments and mortgages, it may not be meaningful to use the information at the individual level. We therefore sum the capital income at the household level.

Demographic information

The information on birth year, education and the linking of parents to their children comes from Statistics Norway demography, family and education registers.

2.4.3 Sample

For both outcomes, our sample consists of couples who had their first child between 1993 and 2007. We restrict the sample to couples where neither parent is younger than 20 - and the woman (man) is no older than 45 (55) - years the year the child was born. We exclude couples who have multiple births. We also exclude couples where either parent is registered as being a single parent the year the child was born, or where either parent

[15] *G* ("Folketrygdens grunnbeløp") is adjusted yearly (or more often) in accordance with changes in the general income level. From January 1 2010, G is NOK 72 881 (apprioximately USD 12 500). It is common to use both 1G and 2G as a lower limit on earnings when defining labor force participation.

Table 2: Descriptive statistics, Wage statistics sample

	1997				2007			
	Mother		Father		Mother		Father	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Participation	0.96	(0.21)	0.99	(0.093)	0.96	(0.21)	0.99	(0.087)
Yearly earnings	203.9	(76.8)	290.5	(111.7)	269.8	(128.5)	409.8	(198.3)
Working hours	34.1	(11.5)	43.6	(15.9)	32.7	(10.6)	43.9	(16.0)
Contracted hours	32.3	(8.47)	36.8	(5.99)	31.5	(8.97)	36.4	(6.18)
Hourly wage	116.4	(29.2)	135.0	(45.5)	157.4	(46.1)	189.8	(90.1)
Age	29.6	(4.21)	31.7	(4.76)	31.7	(4.23)	33.8	(4.76)
Age at first child	29.1	(4.03)	31.2	(4.59)	29.6	(4.09)	31.7	(4.62)
Lower sec. or less	0.15	(0.36)	0.16	(0.37)	0.090	(0.29)	0.13	(0.33)
Upper secondary	0.26	(0.44)	0.35	(0.48)	0.20	(0.40)	0.31	(0.46)
Higher ed. $\leq$ 4 yrs	0.48	(0.50)	0.33	(0.47)	0.53	(0.50)	0.35	(0.48)
Higher ed. $>$ 4 yrs	0.11	(0.31)	0.16	(0.37)	0.18	(0.38)	0.21	(0.41)
N	17866		17866		23308		23308	

Note: Sample is households who had their first child between 1993 and 2007 and where both spouses are registered with employment in Statistics Norway's Wage Statistic in the given year. Participation is a dummy variable indicating whether the individual is registered with income above 2G in the given year. Working hours and contracted hours are measured per week. Working hours is the sum of contracted hours and overtime. Yearly earnings are given in constant 1998 NOK and are measured in 1000s.

already had a child from a previous relationship. Lastly, we only include couples where education status is observed for both parents.

264148 households satisfy these criteria. Then, for each couple we include observations in the four years prior to their first child is born and in the first four years after the child is born. We exclude observations during years in which either parent is still taking education. We also exclude observations for years in which region of residence is observed. We lose 49663 households completely due to these restrictions.

The samples are further only restricted by the availability of data, as described above. The sample used in our analysis of working hours consists of 101519 households, making a total of 269827 household-years. This sample is described in Table 2. The sample used in our analysis of labor force participation consists of 138035 households, with a total of 637465 household-years. This sample is described in Table 3. For reference, descriptive statistics on the whole sample of parents can be found in Appendix Table 11.

In Table 2 we show descriptive statistics for the first and last year of observation on the sample we use in the working hours analysis, i.e., the individual-year observations where the individual is observed in the Wage statistic. The composition of households changed only slightly over the period. The women observed in 2007 work 1.4 less hours on average, have higher wages, are 2.1 years older, and 0.5 years older at first birth. There is also a larger share with higher eduction. The pattern is similar for men. Due to our

Table 3: Descriptive statistics, labor force participation sample

	1997				2007			
	Mother		Father		Mother		Father	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Participation	0.85	(0.36)	0.96	(0.20)	0.91	(0.28)	0.98	(0.14)
Yearly earnings	179.7	(92.6)	280.0	(140.1)	252.9	(145.8)	410.4	(268.6)
Age	28.7	(4.37)	31.0	(4.89)	31.5	(4.27)	33.7	(4.75)
Age at first child	28.3	(4.14)	30.5	(4.66)	29.4	(4.13)	31.6	(4.62)
Lower sec. or less	0.21	(0.40)	0.20	(0.40)	0.11	(0.32)	0.15	(0.36)
Upper secondary	0.32	(0.47)	0.39	(0.49)	0.26	(0.44)	0.36	(0.48)
Higher ed. $\leq$ 4 yrs	0.39	(0.49)	0.28	(0.45)	0.48	(0.50)	0.32	(0.47)
Higher ed. $>$ 4 yrs	0.086	(0.28)	0.13	(0.33)	0.15	(0.36)	0.17	(0.38)
N	55727		55727		43517		43517	

Note: Sample is households who had their first child between 1993 and 2007 and where each spouse is registered with employment in Statistics Norway's Wage Statistic for at least two years in the period 1997-2007. Participation is a dummy variable indicating whether the individual is registered with income above 2G in the given year. Yearly earnings are given in constant 1998 NOK and are measured in 1000s.

sampling of individuals who are observed to become parents by the end of 2007 at the latest, the composition of the sample is of course different in 1997 (where the share of non-parents is 50%) and in 2007 (where the share of non-parents is only 20%). In Table 12 in the Appendix, we divide the sample into those who have children and those who do not yet have children, and we estimate the difference over time for the two groups. In the last column, we also estimate the differences-in-differences for the two groups to see whether the trends have been different over the period for parents and non-parents.

There are some obvious differences when we compare the Wage statistics sample in Table 2 to the descriptive statistics for the population sample of households (all households who fulfill the sample criteria described above, but who are not necessarily observed in the Wage statistic) in Appendix Table 11. This reflects that it is not random who participates and is observed in the Wage statistic. For our analyses of elasticities along the intensive margin, this should be kept in mind.

In Table 3 we show descriptive statistics for the first and last year of observation on the sample we use in the labor force participation analysis, i.e., the sample of households where both parents are observed at least twice in the Wage statistic over the period (but not necessarily in a given year, as wages are extra-/intrapolated other years, based on the existing observations). The changes in the descriptive statistics from 1997 to 2007 for this sample are about the same as in Table 2.

Comparing this sample to the population descriptives in Appendix Table 11, we see that the means are very similar. Naturally, the labor force participation sample has higher labor force participation - and somewhat higher education and income. The parents' age

Table 4: The effect of wages and children on women's working hours

	OLS (1)	OLS-FE (2)	IV (3)	IV-FE (4)	IV'-FE (5)
First child	-0.20***	-0.12***	-0.19***	-0.12***	-0.12***
	(0.0032)	(0.0052)	(0.0032)	(0.0057)	(0.0055)
ln(Own wage)	0.013	-0.33***	0.19***	-0.47**	-0.23
	(0.016)	(0.036)	(0.027)	(0.23)	(0.16)
ln(Own wage) × Child	0.080***	0.014	0.22***	0.13***	0.13***
	(0.021)	(0.025)	(0.027)	(0.031)	(0.024)
ln(Spouse's wage)	0.048***	0.010	0.092***	0.22**	0.12
	(0.0068)	(0.0087)	(0.019)	(0.11)	(0.11)
ln(Spouse wage) × Child	0.022**	-0.0063	-0.0016	-0.046*	-0.031
	(0.0097)	(0.0100)	(0.022)	(0.027)	(0.024)
ln(Capital income)	0.0098***	0.0031***	0.0040***	0.0037***	0.0041***
	(0.00100)	(0.0011)	(0.0010)	(0.0011)	(0.0011)
ln(Cap. income) × Child	-0.0052***	-0.0061***	-0.0088***	-0.0080***	-0.0086***
	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0013)
Individual FE	No	Yes	No	Yes	Yes
N	176150	176150	176130	147555	146858

Note: Each column provides estimates from a linear regression based on Equation 1, the outcome variable being the natural logarithm of weekly working hours. Year dummies and age dummies for both spouses and a dummy for expecting a child and for having a baby younger than one year are included in each regression. Wages are instrumented in specifications (3)-(5) (an alternative instrumental variable is used in specification (5)). Individual fixed effects are included in specifications (2), (4) and (5). Robust standard errors clustered at the individual level are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

at the birth of the first child is about the same. The labor force participation sample is therefore quite representative of the average Norwegian household with children during this period.

2.5 Results

2.5.1 Wage elasticities and the effect of children on female labor supply

Working hours - the intensive margin

Table 4 displays the estimated effect of children, wages, and their interaction, on female working hours. The estimated effect of children on the working hours of women who remain employed ranges from 12 to 20% across specifications. In our most trusted specification in column (4) with both individual fixed effects and instrumented wages, the effect is 12% (this amounts to around 4.5 hours per week for a full time worker). We will concentrate on column (3) and (4) in the following analysis of wage elasticities.

The most important result to note is that women's wage elasticities change substantially once they have children. The increased level of home production increases the substitution effect of female working hours; own wage elasticities become more positive and the cross wage elasticities become more negative. The results are consistent with caring for children being a substitute to female working hours.

The estimated own wage elasticity before children shows a clear pattern. Concentrating on the IV estimates in columns (3) and (4), the elasticity is estimated - very precisely - to be .19 when individual fixed effects are not included. Blau and Kahn (2007) find comparable estimates of .14 for the period 1999-2001 in the US. However, the elasticity turns negative once fixed effects are included. This implies that there are fixed unobserved characteristics that gives a positive correlation between working hours and the mean wages of the IV group in column (3). Identifying the wage elasticities only using within-individual variation gives a negative own wage elasticity of -0.47 in column (4). This means that before children, the income effect dominates the substitution effect. This can be connected to the period before children being a period of large investments (e.g. in housing). It is not uncommon in the literature to find negative own wage elasticities (see Killingsworth (1984) for a review).

The interaction between the logarithm of own wage and the child dummy shows that the own wage elasticity becomes more positive after the child is born (the coefficient is .13). This means that women who have higher wage reduce their working hours less as a result of having children than do women with lower wage. For instance, women who have wages at 10% above the mean work 1.3% more on average after children (meaning that their reduction in hours is 8.3%, rather than 9.6%) compared to women with mean wages - all else equal. Without individual fixed effects we thus see an own wage elasticity after children of .41, with individual fixed effects the elasticity is -.34. This is consistent with the finding of larger own wage elasticities for the group of women with small children in both Blundell et al. (1998) and Blau and Kahn (2007), although these studies do not look at changes within households.

The cross wage elasticity, that is the coefficient on the logarithm of the spouse's wage, is consistently positive across specifications, regardless of the inclusion of individual fixed effects. This means that women whose spouse has a higher wage, tend to work more hours - given their own wage. The cross wage elasticity is .09 and significant at the 1% level when individual fixed effects are not included (specification (3)), about half of the corresponding own wage elasticity. This means that a 10% increase in the spouse's wage causes a .9% increase in hours. Blau and Kahn (2007) estimate the cross wage elasticity to be -.10. The substitution of spouse hours seems therefore to be stronger in the US also before children. Including fixed effects (specification (4)) gives an estimated cross wage elasticity of .22, also about half the absolute size of the corresponding own

Table 5: The effect of wages and children on women's labor force participation

	OLS (1)	OLS-FE (2)	IV (3)	IV-FE (4)	IV'-FE (5)
First child	-0.12***	-0.098***	-0.10***	-0.096***	-0.095***
	(0.0017)	(0.0028)	(0.0018)	(0.0029)	(0.0029)
ln(Own wage)	0.041***	-0.016***	0.19***	0.19***	0.22***
	(0.0039)	(0.0045)	(0.0075)	(0.050)	(0.058)
ln(Own wage) × Child	0.099***	0.077***	0.30***	0.26***	0.26***
	(0.0058)	(0.0059)	(0.010)	(0.011)	(0.012)
ln(Spouse's wage)	-0.0035*	-0.0055*	-0.042***	0.084**	0.13***
	(0.0018)	(0.0032)	(0.0067)	(0.036)	(0.048)
ln(Spouse wage) × Child	0.010***	-0.0021	-0.064***	-0.075***	-0.072***
	(0.0028)	(0.0030)	(0.0096)	(0.011)	(0.011)
ln(Capital income)	0.0058***	0.00092*	0.0039***	0.0022***	0.0023***
	(0.00046)	(0.00055)	(0.00051)	(0.00060)	(0.00061)
ln(Cap. income) × Child	0.0020***	-0.00076	-0.0012	-0.0037***	-0.0041***
	(0.00065)	(0.00068)	(0.00073)	(0.00076)	(0.00079)
Individual FE	No	Yes	No	Yes	Yes
N	418678	418678	409862	386840	385329

Note: Each column provides estimates from a linear regression based on Equation 1, the outcome variable being a dummy for labor force participation. Year dummies and age dummies for both spouses and a dummy for expecting a child and for having a baby younger than one year are included in each regression. Wages are instrumented in specifications (3)-(5). Robust standard errors clustered at the individual level are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

wage elasticity. The cross wage elasticity change by -.046 after having children, going in the opposite direction of the change in own wage elasticity. This means that all else equal, women whose spouse earns more, tend to reduce working hours more after they have children. From the two interaction terms, we thus see that more home production changes the importance of comparative advantage for labor sharing in a direction that is consistent with the Becker model.

Labor force participation - the extensive margin

Table 5 displays the estimated effect of children, wages, and their interaction, on female labor force participation (as defined in 2.4.1). Having children reduces the probability of women participating in the labor market by about 10 percentage points on average, regardless of specification.

As for the results on women's working hours, the most important result to note here is the change in wage elasticities after having children in specification (4). The pattern is the same as for working hours; the substitution effect is stronger and the own wage elasticity becomes more positive, while the cross wage elasticity becomes more negative.

The size of the change is almost double the change we found in working hours elasticities.

For this labor supply measure, the own wage elasticity before children is positive both when we do include and do not include individual fixed effects - and about .2. This is in the lower end of the estimates found elsewhere in the literature - though the most recent estimate in Blau and Kahn (2007) is .3. After children, the elasticity more than doubles in all specifications. The estimated own wage elasticity after having children is thus slightly below .5 when wages are instrumented, regardless of the inclusion of individual fixed effects. The fact that the responsiveness of female labor force participation to wages is much larger after children provides evidence that the elasticity of female labor supply is dependent on how much home production there is, as is suggested by Blau and Kahn (2007).

The cross wage elasticity (the coefficient on the spouse's hourly wage) changes sign according to whether individual fixed effects are included. In column (3) it is negative, indicating that women whose spouse earns a higher hourly wage have a lower probability of participating in paid work. Including fixed effects the estimated cross wage elasticity is positive. According to specification (4), therefore, the typical specialization according to wage incentives is not present before children. The higher the wage of the husband, the higher is the probability that the wife works. This means that before children, changes in wage differences in the family do not lead to within household divergence in participation.

Having children significantly alters the cross wage elasticity with about -.07 (in the IV specifications) - also in accordance with a Beckerian framework.

The coefficients on the interaction terms signify that the probability of going out of the labor force due to having children varies with own and spouse's wage and wage growth. Again looking at specification (4), having a 10% higher wage growth relative to the mean after children means a 2.6 percentage points higher probability of labor force participation. Having a wage growth at 50% higher than the mean cancels out the negative effect of children on participation.[16] The change in participation after children also varies with spouse wage, but the impact is much smaller than that of own wage. All else equal, women whose spouse has 10% higher wage growth, have 0.75 percentage points lower probability of participating after children.

The average effect on female labor force participation is as we see to a large degree heterogeneous with respect to both own wage and spouse wage. The pattern is as predicted in the basic models of household labor supply. Before children (with a smaller amount of household production), the pattern is rather that women work with a higher probability both if she has higher wages herself, and if her husband has higher wages. The mechanisms underlying the results before children are therefore different than the mechanisms presented in the standard models of household behavior.

[16] Admittedly, *if* the elasticity is the same for all incomes, which is not certain.

Table 6: The effect of wages and children on men's working hours

	OLS (1)	OLS-FE (2)	IV (3)	IV-FE (4)	IV'-FE (5)
First child	-0.00059	-0.016***	-0.0021	-0.015***	-0.015***
	(0.0027)	(0.0045)	(0.0027)	(0.0046)	(0.0046)
ln(Own wage)	0.055***	-0.053***	0.10***	-0.27***	-0.17*
	(0.0079)	(0.014)	(0.014)	(0.079)	(0.095)
ln(Own wage) × Child	-0.064***	-0.038***	-0.0084	0.0015	0.00035
	(0.011)	(0.012)	(0.015)	(0.018)	(0.020)
ln(Spouse's wage)	-0.022***	-0.0012	-0.069***	0.10	0.0076
	(0.0065)	(0.0075)	(0.015)	(0.094)	(0.12)
ln(Spouse wage) × Child	-0.00087	-0.021**	-0.039**	-0.040**	-0.040**
	(0.0088)	(0.0087)	(0.016)	(0.018)	(0.019)
ln(Capital income)	0.0040***	0.0024**	0.0032***	0.0031***	0.0030***
	(0.00087)	(0.00098)	(0.00097)	(0.0010)	(0.0010)
ln(Cap. income) × Child	-0.0018	-0.0018*	-0.0035***	-0.0025**	-0.0027**
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Individual FE	No	Yes	No	Yes	Yes
N	176156	176156	176130	147555	146858

Note: Each column provides estimates from a linear regression based on Equation 1, the outcome variable being the natural logarithm of weekly working hours. Year dummies and age dummies for both spouses and a dummy for expecting a child and for having a baby younger than one year are included in each regression. Wages are instrumented in specifications (3)-(5). Robust standard errors clustered at the individual level are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

2.5.2 Wage elasticities and the effect of children on male labor supply

Working hours - the intensive margin

Table 6 displays the estimated effect of children, wages, and their interaction, on male working hours. We find a negative effect of children on men's working hours - at about -1.5% and statistically significant at the 1% level in all the fixed effects specifications.[17] This finding is contrary to what other studies on men's labor market response to fatherhood have found, most of them using data from the US (e.g. Lundberg and Rose (2002)). Our result indicates that specialization in the care for children is less strong in Norwegian households.

In the cross section analysis (specification (3)), the estimated own wage elasticity is .10 and the cross wage elasticity is -.069. As the same pattern is observed for women, we see that households with diverging wages also diverge in hours. Including individual fixed effects in specification (4), the own wage elasticity becomes negative, exactly as for women. The income effect dominates the substitution effect in the period before children.

[17]This amounts to about half an hour a week for the average male childless employee.

Having children does not significantly alter men's own wage elasticity, but the cross wage elasticity becomes more negative (the change is about -.04 and statistically significant at the 5% level in specifications (3) and (4)). The small change in own wage elasticities after children may reflect that home production is a closer substitute to female hours than male hours. The small but significant change in cross wage elasticities does however imply more specialization in line with comparative advantage in households after they have children.

Held together with the results for women, the pattern emerging is that for the average household, a relative change in wages within households leads to changes in hours for both partners. When the woman has a higher wage, she works more and the husband works less. This is also evidence that part of the explanation for men's comparatively small reduction in market hours little after they have children, is that men on average have higher wages than women. The heterogeneity of the response in hours to having children wages shows that men do reduce their market hours more if the wife has higher wages.

Labor force participation - the extensive margin

Table 7 displays the estimated effect of children, wages, and their interaction, on female labor force participation (as defined in 2.4.1). Having children increases the probability of men's labor force participation very little, only 0.53 percentage points. The effect of children on labor force participation in the household is that women on average are less likely and men more likely to participate.

The pattern of wage elasticities is more consistent across specifications for this measure of male labor supply participation. The cross section specification (3) gives a positive own wage elasticity of 0.084 and a negative cross wage elasticity of -0.026. When including fixed effects in specification (4), the cross wage elasticity before children is no longer statistically significant. The other coefficients remains largely unaltered. The pattern is thus the same as for women, and households with diverging wages also diverge in participation probabilities.

After children, wage elasticities are reduced for men, participation is less wage elastic after children. This may reflect the fact that leaving the labor force to stay home with children is not a close alternative for men; home production is not as close a substitute for market production as it is for women (probably the views on gender roles plays an important part here). Rather it is the case that with children, men participate even more certainly because the household has a higher need for a certain level of consumption after children.

The heterogeneity of the response to having children is opposite of the heterogeneity for women. For men, it is those who have larger increases in wages who increase their labor force participation less after children. This may reflect that the men with higher wage

Table 7: The effect of wages and children on men's labor force participation

	OLS (1)	OLS-FE (2)	IV (3)	IV-FE (4)	IV'-FE (5)
First child	0.013***	0.0054***	0.012***	0.0053***	0.0051***
	(0.00098)	(0.0016)	(0.0010)	(0.0016)	(0.0017)
ln(Own wage)	0.039***	0.023***	0.084***	0.11***	0.12***
	(0.0028)	(0.0030)	(0.0047)	(0.021)	(0.029)
ln(Own wage) × Child	-0.015***	-0.013***	-0.026***	-0.025***	-0.022***
	(0.0029)	(0.0026)	(0.0053)	(0.0061)	(0.0065)
ln(Spouse's wage)	0.00035	0.0025	-0.026***	0.022	-0.017
	(0.0016)	(0.0023)	(0.0051)	(0.025)	(0.032)
ln(Spouse wage) × Child	0.0019	0.0019	0.016***	0.012*	0.0085
	(0.0018)	(0.0020)	(0.0055)	(0.0064)	(0.0066)
ln(Capital income)	0.0043***	0.00075*	0.0028***	0.00065	0.00066
	(0.00037)	(0.00041)	(0.00039)	(0.00042)	(0.00042)
ln(Cap. income) × Child	-0.0019***	-0.0017***	-0.0019***	-0.0020***	-0.0020***
	(0.00042)	(0.00043)	(0.00044)	(0.00046)	(0.00046)
Individual FE	No	Yes	No	Yes	Yes
N	418678	418678	409862	386840	385329

Note: Each column provides estimates from a linear regression based on Equation 1, the outcome variable being a dummy for labor force participation. Year dummies and age dummies for both spouses and a dummy for expecting a child and for having a baby younger than one year are included in each regression. Wages are instrumented in specifications (3)-(5). Robust standard errors clustered at the individual level are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

growth are already on higher levels of participation and do not have the same potential to increase it any further.

2.6 Summary of results

Our IV and individual fixed effects analysis shows that women on average reduce their working hours (contingent on being employed) by 12%, and the probability of labor force participation by 10 percentage points, when they have children. For men, the contingent reduction in working hours is 1.5%, whereas the probability of participating increases with half a percentage point. Children thus cause a net reduction in the household's total labor supply.

For both men and women, we find a positive own wage elasticity of working hours before having children in the cross section analysis (.2 for women and .1 for men), and a negative own wage elasticity of working hours before having children when we include individual fixed effects (-.5 for women and -.3 for men). For women, the cross wage elasticity of working hours before children is positive both without and with fixed effects (.1 and .2), whereas for men, the cross wage elasticity before children is negative (at -.07) in the cross section analysis and not statistically significant at conventional levels when individual fixed effects are included. The pattern is slightly different when we look at the wage elasticities of labor force participation: Own wage elasticities before children are positive for both sexes and irrespective of individual fixed effects (about .2 for women and .1 for men). Cross wage elasticities before children are negative for both men and women in the cross section analysis (-.04 for women and -.03 for men), and positive in the fixed effects analysis (.08 for women and .02 (not statistically significant) for men). Summing up, the overall pattern before children is not consistent with husband and wife's market hours being substitutes when we include household fixed effects. The higher the wage of the spouse, the more the wife works, while for men, if the wages of the wife increases, there is no significant change in his hours.

Having children significantly alters the own wage elasticity of both measures of female labor supply, regardless of the inclusion of individual fixed effects. The own wage elasticity of labor force participation more than doubles, as does the own wage elasticity of working hours in the cross section analysis. In the fixed effects analysis, the negative own wage elasticity of female working hours is reduced by 28% after children. The cross wage elasticity of female working hours is not as significantly altered by having children in either specification. However, there is a strong and negative change in the cross wage elasticity of female labor force participation, more than doubling the negative cross wage elasticity before children in the cross section analysis, and practically annulling the positive cross wage elasticity before children in the fixed effects analysis. For men, where having children has a much smaller impact on labor supply, there is no change in the own wage elasticity of working hours, and a negative change at about .025 in the own wage elasticity of labor force participation (reducing the positive own wage elasticity before children by about one fourth). The cross wage elasticity of male working hours becomes significantly more negative after having children (the change is -.04 both without and with individual fixed effects), and the cross wage elasticity of male labor force participation becomes significantly more positive after having children (the change is .016 in the cross section analysis and .012 when including individual fixed effects). The size of the change in own wage elasticities is substantial compared to the relatively small wage elasticities before children. The wage elasticities are still small, and the labor supply response to having children is not very different for individuals with higher wages. There is however a clear pattern that substitution between spouse hours is more important after children. For women who earn 10% higher wages, she works 1.3% more hours while her husband works 0.4% less hours.

2.7 Conclusion

We have shown that the level of home production in the household influences both men and women's level of hours working in the market, and the responsiveness of their respective labor supply to their own and to their spouse's wage. Both men and women in our sample decrease their market hours in response to having children, indicating that both spouses share in the caring for children. Women do however reduce their hours by ten times more than men do - contingent on staying employed - and female labor force participation is reduced by 10 percentage points after having children, whereas men's participation increases slightly.

The change in the responsiveness of labor supply to wages is most prominent for women. This is consistent with caring for children being a closer substitute to female labor supply than to male labor supply. We do however also find that men's working hours become more responsive to their spouse's wage after having children. Held together with the results for women, the pattern emerging is that for the average household, a relative change in wages within households leads to changes in hours for both spouses. If a woman has higher wages, she will work relatively more than other women - and her husband relatively less than their husbands - after having children.

The relatively high wages of women compared to men in Norway thus seem to play a role in changing the traditional tasks of men and women when it comes to child rearing.

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Table 8: First stage results - alternative IV

	Mother wage	Father wage	Mother wage × C1	Father wage × C1
Panel A: OLS				
Mean wage in mother's IV group	0.75***	0.16***	-0.05***	-0.03***
	(0.01)	(0.01)	(0.00)	(0.00)
Mean wage in father's IV group	0.09***	0.70***	-0.03***	-0.02***
	(0.01)	(0.01)	(0.00)	(0.00)
Mean wage in mother's IV group × C1	-0.03***	0.04***	0.80***	0.26***
	(0.01)	(0.01)	(0.01)	(0.01)
Mean wage in father's IV group × C1	0.01	0.00	0.14***	0.74***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	175397	175399	175397	175399
	Mother wage	Father wage	Mother wage × C1	Father wage × C1
Panel B: FE				
Mean wage in mother's IV group	0.15***	0.07***	-0.39***	-0.11***
	(0.01)	(0.01)	(0.02)	(0.01)
Mean wage in father's IV group	0.04***	0.12***	-0.06***	-0.36***
	(0.01)	(0.01)	(0.01)	(0.01)
Mean wage in mother's IV group × C1	0.02**	-0.00	0.80***	0.23***
	(0.01)	(0.01)	(0.01)	(0.01)
Mean wage in father's IV group × C1	-0.01	0.07***	0.12***	0.75***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	175397	175399	175397	175399

Note: Each column provides estimates from a regression based on Equation 2, the outcome variable corresponding to the column header. Household capital income, a dummy for having a first child and the interaction between them are included in each regression. So are year dummies and age dummies for both spouses, plus a dummy for expecting a child and for having a baby younger than one year. In the lower panel individual fixed effects are included. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 9: First stage results - IV for imputed wages

	Mother wage	Father wage	Mother wage × C1	Father wage × C1
Panel A: OLS				
Mean wage in mother's IV group	0.81***	0.17***	-0.03***	-0.01**
	(0.01)	(0.01)	(0.00)	(0.00)
Mean wage in father's IV group	0.11***	0.80***	-0.01***	0.02***
	(0.01)	(0.01)	(0.00)	(0.00)
Mean wage in mother's IV group × C1	0.02**	0.04***	0.89***	0.22***
	(0.01)	(0.01)	(0.01)	(0.01)
Mean wage in father's IV group × C1	-0.01	-0.00	0.11***	0.77***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	514567	503134	514567	503134
	Mother wage	Father wage	Mother wage × C1	Father wage × C1
Panel B: FE				
Mean wage in mother's IV group	0.26***	0.06***	-0.29***	-0.04***
	(0.03)	(0.01)	(0.02)	(0.01)
Mean wage in father's IV group	0.04***	0.31***	-0.04***	-0.23***
	(0.01)	(0.02)	(0.01)	(0.01)
Mean wage in mother's IV group × C1	0.03***	0.01	0.86***	0.20***
	(0.01)	(0.01)	(0.01)	(0.01)
Mean wage in father's IV group × C1	-0.01	0.05***	0.12***	0.81***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	514567	503134	514567	503134

Note: Each column provides estimates from a regression based on Equation 2, the outcome variable corresponding to the column header. Household capital income, a dummy for having a first child and the interaction between them are included in each regression. So are year dummies and age dummies for both spouses, plus a dummy for expecting a child and for having a baby younger than one year. In the lower panel individual fixed effects are included. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 10: First stage results - alternative IV for imputed wages

	Mother wage	Father wage	Mother wage × C1	Father wage × C1
Panel A: OLS				
Mean wage in mother's IV group	0.78***	0.17***	-0.06***	-0.02***
	(0.01)	(0.01)	(0.00)	(0.00)
Mean wage in father's IV group	0.10***	0.77***	-0.03***	-0.00*
	(0.01)	(0.01)	(0.00)	(0.00)
Mean wage in mother's IV group × C1	-0.04***	0.06***	0.82***	0.27***
	(0.01)	(0.01)	(0.01)	(0.01)
Mean wage in father's IV group × C1	0.03***	-0.04***	0.15***	0.74***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	512641	501339	512641	501339
	Mother wage	Father wage	Mother wage × C1	Father wage × C1
Panel B: FE				
Mean wage in mother's IV group	0.17***	0.06***	-0.42***	-0.08***
	(0.02)	(0.01)	(0.01)	(0.01)
Mean wage in father's IV group	0.02**	0.15***	-0.07***	-0.41***
	(0.01)	(0.01)	(0.01)	(0.01)
Mean wage in mother's IV group × C1	0.01	0.01	0.82***	0.23***
	(0.01)	(0.01)	(0.01)	(0.01)
Mean wage in father's IV group × C1	0.01	0.05***	0.14***	0.80***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	512641	501339	512641	501339

Note: Each column provides estimates from a regression based on Equation 2, the outcome variable corresponding to the column header. Household capital income, a dummy for having a first child and the interaction between them are included in each regression. So are year dummies and age dummies for both spouses, plus a dummy for expecting a child and for having a baby younger than one year. In the lower panel individual fixed effects are included. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 11: Descriptive statistics, whole sample

	1997				2007			
	Mother		Father		Mother		Father	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Participation	0.77	(0.42)	0.92	(0.26)	0.78	(0.41)	0.93	(0.25)
Yearly earnings	160.6	(100.1)	271.5	(888.3)	211.8	(150.2)	380.3	(1076.1)
Age	28.5	(4.55)	30.9	(5.14)	30.6	(4.66)	33.2	(5.26)
Age at first child	28.0	(4.31)	30.4	(4.90)	28.6	(4.47)	31.2	(5.10)
Lower sec. or less	0.27	(0.44)	0.26	(0.44)	0.22	(0.41)	0.23	(0.42)
Upper secondary	0.34	(0.47)	0.40	(0.49)	0.27	(0.44)	0.38	(0.48)
Higher ed. $\leq$ 4 yrs	0.33	(0.47)	0.24	(0.42)	0.40	(0.49)	0.26	(0.44)
Higher ed. $>$ 4 yrs	0.068	(0.25)	0.10	(0.30)	0.12	(0.32)	0.14	(0.34)
N	90023		90023		80486		80486	

Note: Sample is households who had their first child between 1993 and 2007. Participation is a dummy variable indicating whether the individual is registered with income above 2G in the given year. Yearly earnings are given in constant 1998 NOK and are measured in 1000s.

Table 12: Alternative descriptive statistics, Wage statistics sample

	1997 parents		1997 non-parents		Diff. time parents		Diff. time non-parents		Diff-in-diff	
	Mean	SD	Mean	SD	Estimate	SE	Estimate	SE	Estimate	SE
Mothers										
- participation	0.94	(0.24)	0.97	(0.17)	0.0096***	(0.0029)	0.0066**	(0.0029)	0.0030	(0.0046)
- yearly earnings	191.6	(76.0)	216.0	(75.8)	75.9***	(1.53)	63.1***	(1.54)	12.8***	(2.41)
- working hours	31.6	(10.6)	36.6	(11.9)	1.21***	(0.14)	-4.15***	(0.20)	5.36***	(0.24)
- contracted hours	30.5	(8.88)	34.1	(7.63)	1.06***	(0.12)	-2.95***	(0.14)	4.01***	(0.19)
- hourly wage	117.6	(28.8)	115.2	(29.5)	40.1***	(0.54)	40.9***	(0.63)	-0.78	(0.88)
- age	30.9	(4.03)	28.3	(3.97)	1.12***	(0.053)	1.97***	(0.072)	-0.85***	(0.091)
- age at first child	28.3	(3.96)	29.9	(3.94)	1.13***	(0.052)	0.18**	(0.072)	0.94***	(0.089)
- lower sec. or less	0.18	(0.38)	0.13	(0.34)	-0.086***	(0.0042)	-0.055***	(0.0056)	-0.031***	(0.0071)
- upper secondary	0.26	(0.44)	0.26	(0.44)	-0.051***	(0.0054)	-0.069***	(0.0076)	0.018**	(0.0093)
- higher ed. $\leq$ 4 yrs	0.46	(0.50)	0.50	(0.50)	0.062***	(0.0064)	0.033***	(0.0090)	0.029***	(0.011)
- higher ed. $>$ 4 yrs	0.099	(0.30)	0.12	(0.32)	0.075***	(0.0046)	0.092***	(0.0063)	-0.017**	(0.0078)
Fathers										
- participation	0.99	(0.078)	0.99	(0.11)	-0.0014	(0.0011)	0.0029	(0.0018)	-0.0043**	(0.0020)
- yearly earnings	296.3	(110.0)	284.9	(113.0)	115.5***	(2.27)	116.8***	(2.63)	-1.25	(3.68)
- working hours	43.1	(15.4)	44.1	(16.3)	0.61***	(0.20)	0.46	(0.30)	0.15	(0.35)
- contracted hours	36.9	(6.10)	36.8	(5.89)	-0.51***	(0.078)	-0.48***	(0.11)	-0.026	(0.14)
- hourly wage	136.9	(44.5)	133.1	(46.3)	54.1***	(1.03)	51.9***	(1.09)	2.18	(1.64)
- age	33.1	(4.63)	30.4	(4.50)	1.16***	(0.061)	1.89***	(0.082)	-0.73***	(0.10)
- age at first child	30.5	(4.56)	32.0	(4.50)	1.16***	(0.059)	0.099	(0.082)	1.06***	(0.10)
- lower sec. or less	0.17	(0.38)	0.15	(0.35)	-0.042***	(0.0045)	-0.025***	(0.0062)	-0.017**	(0.0077)
- upper secondary	0.36	(0.48)	0.34	(0.47)	-0.040***	(0.0061)	-0.047***	(0.0084)	0.0069	(0.010)
- higher ed. $\leq$ 4 yrs	0.32	(0.47)	0.35	(0.48)	0.031***	(0.0061)	0.00085	(0.0086)	0.030***	(0.011)
- higher ed. $>$ 4 yrs	0.15	(0.36)	0.17	(0.37)	0.051***	(0.0050)	0.071***	(0.0071)	-0.020**	(0.0086)
N	8860		9006		27487		13687		41173	

Note: Sample is households who had their first child between 1993 and 2007 and where both spouses are registered with employment in Statistics Norway's Wage Statistic in the given year. Participation is a dummy variable indicating whether the individual is registered with income above 2G in the given year. Working hours and contracted hours are measured per week. Working hours is the sum of contracted hours and overtime. Yearly earnings are given in constant 1998 NOK and are measured in 1000s.

Table 13: Alternative descriptive statistics, LFP sample

	1997 parents		1997 non-parents		Diff. time parents		Diff. time non-parents		Diff-in-diff	
	Mean	SD	Mean	SD	Estimate	SE	Estimate	SE	Estimate	SE
Mothers										
- participation	0.81	(0.39)	0.89	(0.31)	0.095***	(0.0028)	0.063***	(0.0035)	0.033***	(0.0047)
- yearly earnings	164.4	(92.0)	194.0	(90.8)	85.6***	(1.06)	69.6***	(1.14)	16.0***	(1.71)
- age	30.2	(4.14)	27.4	(4.12)	1.74***	(0.034)	2.66***	(0.049)	-0.92***	(0.060)
- age at first child	27.6	(4.07)	29.0	(4.10)	1.72***	(0.033)	0.87***	(0.049)	0.84***	(0.059)
- lower sec. or less	0.24	(0.43)	0.18	(0.38)	-0.12***	(0.0030)	-0.062***	(0.0044)	-0.063***	(0.0053)
- upper secondary	0.32	(0.46)	0.32	(0.47)	-0.059***	(0.0037)	-0.066***	(0.0055)	0.0068	(0.0066)
- higher ed. $\leq$ 4 yrs	0.37	(0.48)	0.41	(0.49)	0.11***	(0.0040)	0.063***	(0.0059)	0.052***	(0.0071)
- higher ed. $>$ 4 yrs	0.077	(0.27)	0.094	(0.29)	0.069***	(0.0026)	0.065***	(0.0037)	0.0042	(0.0046)
Fathers										
- participation	0.97	(0.16)	0.95	(0.22)	0.0078***	(0.0012)	0.033***	(0.0024)	-0.025***	(0.0025)
- yearly earnings	291.7	(131.0)	269.1	(147.4)	122.0***	(1.79)	129.2***	(2.17)	-7.13**	(2.99)
- age	32.5	(4.72)	29.5	(4.61)	1.67***	(0.038)	2.65***	(0.055)	-0.97***	(0.068)
- age at first child	29.9	(4.65)	31.1	(4.59)	1.65***	(0.038)	0.85***	(0.055)	0.79***	(0.067)
- lower sec. or less	0.22	(0.41)	0.19	(0.39)	-0.068***	(0.0031)	-0.040***	(0.0046)	-0.028***	(0.0055)
- upper secondary	0.39	(0.49)	0.39	(0.49)	-0.030***	(0.0039)	-0.034***	(0.0058)	0.0039	(0.0070)
- higher ed. $\leq$ 4 yrs	0.27	(0.44)	0.29	(0.45)	0.053***	(0.0037)	0.018***	(0.0054)	0.036***	(0.0066)
- higher ed. $>$ 4 yrs	0.12	(0.33)	0.13	(0.34)	0.045***	(0.0029)	0.056***	(0.0042)	-0.011**	(0.0051)
N	27017		28710		61324		37920		61177	

Note: Sample is households who had their first child between 1993 and 2007 and where each spouse is registered with employment in Statistics Norway's Wage Statistic for at least two years in the period 1997-2007. Participation is a dummy variable indicating whether the individual is registered with income above 2G in the given year. Yearly earnings are given in constant 1998 NOK and are measured in 1000s.

Table 14: Alternative descriptive statistics, whole sample

	1997 parents		1997 non-parents		Diff. time parents		Diff. time non-parents		Diff-in-diff	
	Mean	SD	Mean	SD	Estimate	SE	Estimate	SE	Estimate	SE
Mothers										
- participation	0.71	(0.45)	0.83	(0.38)	0.063***	(0.0027)	-0.014***	(0.0033)	0.076***	(0.0044)
- yearly earnings	143.9	(99.1)	177.4	(98.2)	66.4***	(0.84)	39.1***	(0.93)	27.4***	(1.33)
- age	29.9	(4.31)	27.0	(4.32)	1.22***	(0.028)	1.75***	(0.038)	-0.53***	(0.047)
- age at first child	27.3	(4.23)	28.6	(4.28)	1.26***	(0.027)	0.000058	(0.038)	1.26***	(0.046)
- lower sec. or less	0.30	(0.46)	0.23	(0.42)	-0.090***	(0.0027)	-0.0016	(0.0036)	-0.088***	(0.0045)
- upper secondary	0.33	(0.47)	0.34	(0.47)	-0.058***	(0.0028)	-0.074***	(0.0040)	0.016***	(0.0049)
- higher ed. $\leq$ 4 yrs	0.31	(0.46)	0.35	(0.48)	0.093***	(0.0030)	0.031***	(0.0041)	0.062***	(0.0051)
- higher ed. $>$ 4 yrs	0.061	(0.24)	0.075	(0.26)	0.055***	(0.0018)	0.045***	(0.0024)	0.010***	(0.0030)
Fathers										
- participation	0.93	(0.25)	0.92	(0.28)	0.0018	(0.0015)	0.018***	(0.0023)	-0.016***	(0.0027)
- yearly earnings	284.7	(1235.5)	258.3	(224.1)	97.8***	(5.23)	114.8***	(10.1)	-16.9	(10.4)
- age	32.4	(4.96)	29.4	(4.88)	1.33***	(0.031)	1.97***	(0.043)	-0.64***	(0.053)
- age at first child	29.8	(4.88)	31.0	(4.85)	1.37***	(0.031)	0.21***	(0.043)	1.15***	(0.053)
- lower sec. or less	0.28	(0.45)	0.24	(0.43)	-0.055***	(0.0027)	-0.0038	(0.0037)	-0.051***	(0.0046)
- upper secondary	0.39	(0.49)	0.40	(0.49)	-0.019***	(0.0030)	-0.026***	(0.0042)	0.0068	(0.0052)
- higher ed. $\leq$ 4 yrs	0.22	(0.42)	0.25	(0.43)	0.037***	(0.0027)	-0.0053	(0.0037)	0.042***	(0.0046)
- higher ed. $>$ 4 yrs	0.099	(0.30)	0.11	(0.31)	0.037***	(0.0020)	0.035***	(0.0027)	0.0026	(0.0034)
N	45060		44963		106253		64256		76974	

Note: Sample is households who had their first child between 1993 and 2007. Participation is a dummy variable indicating whether the individual is registered with income above 2G in the given year. Yearly earnings are given in constant 1998 NOK and are measured in 1000s.

Chapter 3

Parenthood wage penalties in a double income society

Sara Cools[1] Marte Strøm[2]

Abstract We estimate parenthood wage penalties using panel data for Norwegian employees in the period 1997-2007. The Norwegian institutional setting is one of high female labor force participation and family friendly welfare policies like publicly provided child care, paid parental leave and job protection during the absence. Nevertheless, we find substantial wage penalties to motherhood, ranging from a 1.5% wage reduction for women with lower secondary education to 4.7% for women with more than four years of higher education. The wage penalties we find for women can not be explained by years spent not working or on maternity leave, nor by moving to part time work or public sector employment subsequent to having children. The motherhood wage penalties are however larger for women with higher education and for women who were working full time and in the private sector before having children. Contrary to most results found using U.S. data and previous research for Norway, we find a wage penalty also to fatherhood for men with more than lower secondary education at about .5%. Also for men, the penalty is greater for those who work full time and in the private sector. A substantial share of the fatherhood wage penalty can be explained by paternity leave.

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3.1 Introduction

The economic dynamic of the household changed greatly over the 20th century, as both female participation in market work rose and total fertility declined. Now, even as total fertility fell dramatically in most OECD countries, the population share having at least one child did not fall as dramatically. The two income household with children is increasingly becoming the norm rather than the exception.

In Scandinavia, the combination of work and family is encouraged through various policies securing child care, paid parental leave and job protection. In addition, not only the two career household, but also the two *carer* household, is encouraged. Men are increasingly participating in the rearing of children. As a result, many would argue, Scandinavia has both the highest female labor force participation and among the highest fertility rates in the OECD.[3]

There is a large literature on the negative career effects of having children for mothers (e.g. Waldfogel (1997); Budig and England (2001)), and some evidence of a positive career effect for men (e.g. Lundberg and Rose (2002); Simonsen and Skipper (2008)), consistent with women and men specialising in different tasks after children; child rearing and market production respectively. Our context is however different from other countries, with a female labor force participation almost as high as for men (80% in 2007 in the age group 25-64)[4], and the gender wage gap is among the narrowest in the OECD countries. We show that in this context, the motherhood penalties are still considerable, but we also find that men experience a negative private cost from fatherhood.

This is the first broad panel data study of parenthood wage effects in Norway. We use data from official Norwegian registries on wages and income, covering about 70% of the working population in the years from 1997 to 2007. Using individual fixed effects estimation on a sample of individuals who are observed to have at least one child by 2008, we find a substantial wage penalty for women - ranging from -1.4% for women with lower secondary education to -4.6% for women with higher education, higher degree.[5]. Contrary most other studies, we find negative (though comparatively small) effects of having children on men's wages - about .5% for men with upper secondary education or more. This is consistent with an increasing role of fathers as care givers.

The estimated penalty for women is only partly explained when we include measures of experience, parental leave, working part time and sector of employment. For men on the other hand, variation in parental leave explains half of the fatherhood wage penalty. We find the largest penalties to parenthood in the private sector, for full time workers, and for those who take the longest leave.

[3]Source: OECD.stat.

[4]Source: OECD.stat. The corresponding number for men is 87%.

[5]The numbers are comparable to other studies on Norway (Hardoy and Schøne, 2008; Harkness and Waldfogel, 2003)

The rest of the paper is organized as follows: Section 3.2 presents the literature on wage penalties (and premia) to parenthood. Section 3.3 presents our empirical strategy. In Section 3.4 we give a description of our data. In Section 3.5 we present the results of our baseline specification. In Section 3.6 we try to identify some channels through which the parenthood wage penalty might work, and in Section 3.7 we investigate the variation in the penalty across different subgroups. Section 3.8 concludes.

3.2 The literature on the wage penalties to parenthood

The economic literature offers several explanations for why we observe a motherhood wage penalty.[6] The most common explanation considers the effect of children on women's productivity, which again will be reflected in their wages. Two main mechanisms for this effect have been proposed. First, childbearing and child rearing may cause a comparative reduction in mothers labor market experience, both through periods out of the labor market and through periods of reduced working hours. This will adversely affect mothers' accumulation of human capital (Mincer and Polachek, 1974). Second, in the framework of Becker (1985), even for women working the same number of hours, mothers may put in less effort per hour because they spend more effort during their time at home.

Other explanations do not evoke an effect of children on women's productivity: Mothers may earn lower wages than non-mothers due to employers' discrimination or because they seek employment in "mother-friendly" jobs, offering for instance greater flexibility at the price of lower wages.

There is substantial empirical evidence on a negative relationship between having children and labor market outcomes like wages and labor supply (e.g Korenman and Neumark (1992); Waldfogel (1997); Budig and England (2001); Lundberg and Rose (2002)). The focus of this literature has mainly been on the effects of motherhood.

There are some studies on the effects of fatherhood on men's wages and working hours. Millimet (2000), Lundberg and Rose (2002) and Simonsen and Skipper (2008) find a positive effect of having children both on men's wages and working hours using U.S. data. Astone et al. (2010) document a heterogeneous response for fathers; young men work more while older men work less when they become fathers. Wilde et al. (2010) find small negative effects of fatherhood on men's wages (although they are not significant in most specifications).

[6] These explanations all apply to *general* parenthood penalties (in the case of observed penalties to fatherhood).

3.2.1 The role of human capital

Most studies attribute a large part of the effect to the adverse human capital effects of career breaks following the birth of a child, in the framework of Mincer and Polachek (1974). Several authors have shown that when controlling for *experience* - measured as time in active employment, net of career breaks and/or adjusted for part time - a large part of the wage penalty is explained. The penalty is consequently interpreted as a *human capital effect* (Waldfogel, 1997; Lundberg and Rose, 2000; Budig and England, 2001; Datta Gupta and Smith, 2002; Anderson et al., 2002; Wilde et al., 2010).

Using Danish data, Datta Gupta and Smith (2002) find no negative effect on wages of having children, apart from the negative effect of a career break. Similarly, Lundberg and Rose (2000), using U.S. data, find no wage effect of children for women who are continuously attached to the labor market. Anderson et al. (2002), also using U.S. data, find that the number of years out of the labor force explains 30% of the motherhood wage penalty on average. For college educated women, the whole penalty is explained by years out of the labor force. In most other studies, an unexplained wage penalty remains, even after controlling for experience.

Wilde et al. (2010) find larger wage penalties for women with higher skills. In so far as human capital is more important in jobs held by higher educated individuals, such a pattern is consistent with a human capital explanation to the motherhood wage penalty.

3.2.2 The role of effort

The Becker (1985) theory of a conflict between effort in home production and effort at work is also supported in correlation studies. Several authors have found a negative relationship between housework load and wages (Coverman, 1983; Shelton and Firestone, 1989; McAllister, 1990; Hersch and Stratton, 1997).

In addition, it is not clear whether we can interpret the length of a career break as a human capital effect, or whether it reflects an individual's propensity to spend more effort at home relative to at work after returning to work. Using Swedish data, Albrecht et al. (1999) find that the type of career break (paid parental leave, unpaid care leave, periods spent abroad, shorter leave periods, unemployment, military service) matters for the effect on wages, and that the effects vary by gender. They find no effect of maternity leave and a negative effect of paternity leave.[7] They interpret this as a signalling effect of parental leave, rather than a human capital effect. As mothers are already expected by employers to take leave for a considerable period of time, maternity leave is not a strong signal to the employer of their type. For men, on the other hand, the length of parental leave is a strong signal.

[7]This applies to their cross section analysis. The results are weakened in individual fixed effects estimation.

3.2.3 Handling endogeneity

There are several endogeneity issues when estimating the effect of having children on career outcomes. First, *who* becomes parents may not be random. Especially for men, the issue of positive selection into marriage and fatherhood is debated (Korenman and Neumark, 1991; Gupta et al., 2007; Antonovics and Town, 2004; Krashinsky, 2004). Positive selection into parenthood gives an upward bias in the estimated effect of having children when comparing parents to non-parents.

The most common measure used in the literature to correct for selection is to include individual fixed effects, and thereby take out systematic differences in the wage levels of parents and non-parents (e.g. Waldfogel (1997); Hersch and Stratton (1997); Budig and England (2001); Anderson et al. (2002)). However, if individuals with higher wage growth also have a higher propensity to become parents, results will still be biased with fixed effects estimation.

Another solution to the problem of positive selection on both wage levels and wage growth is to use a sample consisting only of individuals who are eventually observed to become parents, as is done by Wilde et al. (2010).

Using individual fixed effects in a sample of individuals who become parents at one point during the time window means that the effect of children is estimated as the average difference in a given outcome between those who have had a child and those who have not yet had a child. If the timing of childbearing is endogenous to an individual's career, estimates will again be biased.

Postponing childbearing is found to reduce the negative career effects of having children (Hofferth, 1984; Taniguchi, 1999; Buckles, 2008; Miller, 2011). Since there are strong economic incentives to wait, the timing of children can not be assumed to be random.

If individuals with steeper wage growth have children at later ages, the estimated effect of children will be exaggerated in a fixed effects analysis conducted on a sample of parents and parents-to-be. Wilde et al. (2010) address the endogeneity in the timing of childbearing by running separate analyses of the wage effects for different skill levels (defined by Armed Forces Qualification Scores taken at ages 14-21). If the timing of children is random within a skill group, separate analyses will yield unbiased estimates.

A few studies estimate wage effects of children using instrumental variables (see for instance Angrist and Evans (1998) and Miller (2011)). Twin births and sibling sex composition are popular instruments, but can only be used for estimating effects of second and third child (or later), respectively. Miscarriage can be used as an instrument for the effect of a first child - but is generally harder to observe.

When good instrumental variables are not readily available it is important to compare individuals who are on similar wage paths before they have children.

3.2.4 Parenthood wage penalties in Norway

Petersen et al. (2007) and Hardoy and Schøne (2008) study the association between parenthood and wages in Norway. Their results are not readily comparable to those presented in this paper, as none of them use individual fixed effects and both include individuals who are not observed to become parents in the sample. Both studies find somewhat smaller point estimates than we do of a motherhood wage penalty, and they both find a small positive effect of children on men's wages.

Petersen et al. (2007) use data for the private sector in Norway, for the years 1980 to 1997. Using fixed effects at the occupational level they study in particular how the parenthood penalties - or, in men's case, a premium - are explained by occupational sorting. They find that the within-job penalty for women disappears over the period, whereas there is a small and stable premium for men.

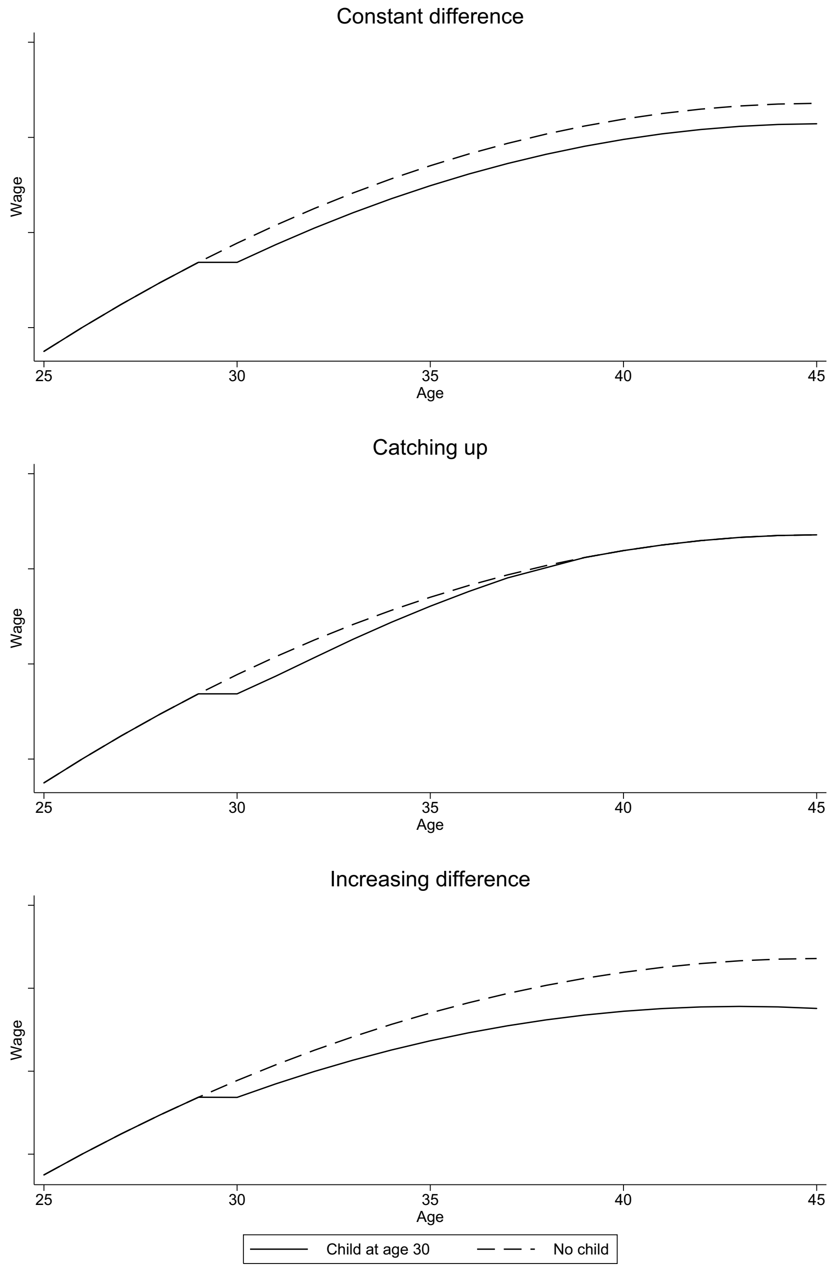
Hardoy and Schøne (2008) run separate cross-section analyses of the years 1997 and 2003. They find that mothers with two children have 3% lower wages on average than women with no children, and that the number is stable over the period. Using quantile regressions, they find that the motherhood wage penalty is largest at the top of the income distribution. Fathers of two children have on average 5% higher wages than men who do not have children.

3.3 Estimating the wage penalty to parenthood

The concept of a wage penalty to having children is illustrated in Figure 1. The wage of an individual is assumed to follow a certain path, illustrated by the dotted line, along which the wage would continue developing if the individual were not to have children. Then (at age 30) the individual becomes a parent and his or her wages are put off from the original path. Wages then continue developing along a different path, illustrated by the solid line. What is usually referred to as the *parenthood wage penalty* is the distance at a certain point in time between an individual's actual wage and the counterfactual wage at that point. The *life time wage cost* of having children is the area between the two lines over the rest of the individual's working life.

Three stylized development scenarios are illustrated in Figure 1. In the uppermost panel, wages are set back initially and continue to grow at the same rate as the counterfactual wage (the dotted reference path). This means the individual's wages never catch up with what his or her wages would have been in the absence of children, but remain at a constant distance. In the middle panel, though wages are set back initially they grow faster than the reference path. The penalty thus becomes smaller over time, until wages have caught up with the reference path - and maybe surpasses it. The lowermost panel illustrates the case where wages continue to grow more slowly than the reference path after the initial setback, and the penalty thus grows over time.

Figure 1: Illustration of the counterfactual wage path and the actual wage path of parents



3.3.1 Empirical strategy

As the individuals' counterfactual wage paths can not be observed, we need to compare parents to non-parents who are expected to follow similar wage paths - all else equal - in order to identify the wage penalty to parenthood. The identifying assumption in this paper, is that the wages of individuals with the same level of education, conditional on age and individual fixed effects, who eventually have children - but who do not have children yet - follow the same path as the wages of those who have children would have done in the absence of children. Our empirical strategy is explained more in detail below.

In order to avoid the problem of selection into parenthood, we exclude individuals who are not observed to have had children by the end of 2007. Lundberg and Rose (2000) and Wilde et al. (2010) also correct for selection into parenthood by restricting the sample to contain only people who eventually become parents.

In all our estimations we include individual fixed effects in order to take out time-invariant unobservable heterogeneity that is correlated both with the average wage level and with the propensity to have children at different ages. However, as discussed in Section 3.2.3, bias may be caused also by time-*variant* heterogeneity in unobservables: People who have children early may be systematically different from people who have children late, in ways that matter for their wage development. We thus need to compare individuals who are expected to follow similar wage paths, all else equal. Wilde et al. (2010) address this problem by running separate analyses for four quartiles in the skill distribution, as measured by Armed Forces Qualification Scores taken at ages 14-21. We do not have such ability measures, so we need to rely on observed realised education. As an individual's ultimate education level may be influenced by childbearing, we face a trade-off between an education measure that truly reflects the individual's type and wage path on the one hand, and an education measure not too tainted by endogeneity on the other.

Our strategy is to use individuals' completed education at the age of 27 and in addition only include individuals who had their first child after the age of 23. In this way we are able to distinguish many of those who end up having higher education.[8] Our results are robust to including individuals who had children earlier and also to using the last observation on completed education. The different education groups we look at are 1) Lower secondary education or less, 2) Upper secondary education, 3) Higher education, lower degree (four years or less of higher education) and 4) Higher education, higher degree (more than four years of higher education).

The resulting identifying assumption for a causal interpretation of our results is that *within an education group, there is no systematic difference in the wage growth of those*

[8] Measuring education at an earlier stage, we lose a lot of observations of higher education, higher degree. Picking a later age of first birth, we lose observations in the lower education groups.

who have children at different points in time, other than what is caused by the event of having children itself.

Following Wilde et al. (2010), we test the issue of reverse causality - what if people choose to start a family at a moment when they observe (or expect) wage growth to slow down? - by a "simplified type of causality test", and find that, on the contrary, wages grow faster just before the woman gets pregnant.[9]

3.3.2 Baseline specification

The baseline specification takes the form:

$$\begin{aligned} \ln w_{it} = &\pi_a Child[1\text{-}5]_{it} + \pi_b Child[6\text{-}10]_{it} + \pi_c Child[11\text{-}15]_{it} \\ &+ \rho_a Pregnant_{it} + \rho_b Baby_{it} + \rho_c Child[>15]_{it} \\ &+ \gamma X_{it} + \delta f(Age_{it}) + e_t + v_i + u_{it}, \end{aligned} \tag{1}$$

where i and t are the individual and year indicators, respectively. w denotes hourly wages. The $Child[*]$ variables are dummies for the age categories of the first child. $Child[1\text{-}5]_{it}$ is a variable taking a value between 0 and 1 according to how much of year t individual i's first child is 1 to 5 years old.[10]

We wish to estimate the effect of having a child on wages *after* the first child turns one, as family policies in Norway guarantee parents wage compensated parental leave during the first year. The variable $Baby_{it}$, taking a value between 0 and 1 according to how much of that year the individual had a first-born baby younger than 1 year, takes out the variation in wages caused by this period. Similarly, we take out the variation in wages caused by pregnancy or the expectation of a child by using the variable $Pregnant_{it}$, which takes a value between 0 and 0.75 according to how much of the year was spent "in pregnancy".[11]

X is a vector of controls for subsequent children (six variables for each of child number 2 and 3, constructed identically to those used for the first child, and a sole dummy indicating having more than three children).[12] $f(Age)$ is a fourth order polynomial in each parent's

[9] We test this by including a dummy variable for "expecting" a child in the sense of having a child less than two years later. For both men and women, in all education groups and prior to all three children, the estimated coefficients on these variable are positive - and quite often statistically significant. The results can be seen in Tables 10 and 11 in the Appendix.

[10] $Child[6\text{-}10]_{it}$ is a variable taking a value between 0 and 1 according to how much of year t individual i's first child is 6 to 10, and $Child[11\text{-}15]_{it}$ is a variable taking a value between 0 and 1 according to how much of year t individual i's first child is 11 to 15 years old.

[11] Both variables are included also in the analyses on men.

[12] Including variables for subsequent children may introduce selection bias to the equation, as the measures discussed at the beginning of this section were taken to enhance the causal interpretation of the estimated impact of the *first* child only. Results are essentially unchanged when controls for subsequent children are not included. Also, in the sample of individuals who are observed to have at least two children by 2008, the estimated impact of the first child is essentially the same as in our baseline specification. (Tables available from the authors upon request.)

age. e_t is time fixed effects and v_i is individual fixed effects.

Our main interest lies with the parameters π_a, π_b and π_c, the effect on wages of having a first child aged 1 to 5 years, 6 to 10 years and 11 to 15 years, respectively. The πs are thus identified by comparing the wages of parents with the wages of the individuals with the same level of education who have not yet have children, taking out individual fixed effects and the variation due to age, pregnancies and caring for babies. As the dependent variable is the natural logarithm of the wage, the interpretation of π_a is that having a child between 1 and 5 years of age reduces wages by $100\pi_a$ percent. A change in π_a of -0.01 means that the effect of children on wages is one percentage point more negative. In terms of Figure 1, for (weakly) negative values of the pis, $\pi_a = \pi_b = \pi_c$ would correspond to the constant effect over time of the uppermost panel, $|\pi_a| > |\pi_b| > |\pi_c|$ corresponds to the scenario of parents' wages catching up with their original wage path of the middle panel, and $|\pi_a| < |\pi_b| < |\pi_c|$ corresponds to the case where parents' wages keep deteriorating relative to the wages of non-parents, as illustrated in the lowermost panel.

3.4 Data

Descriptive statistics for our sample of women and men are given in Table 1. The first 8 rows in each panel gives the mean values of the variables observed in 2002. The next five rows give the means for the variables we use to split samples in Section 3.7, and are thus for the sample of parents with children born after 1993. These are constants for the individual during the time window.

There are notable differences, both between men and women on average, and between education groups. The women in our sample on average have lower wages, fewer and younger children and are themselves younger and less experienced in the labor market, than the men in our sample. They have on the other hand accumulated more parental leave, and a much greater propensity to part time and public sector work.

The spread in wages is much greater for men than for women. Higher education pays off in terms of higher hourly wage for both genders, and by about as much, hence it is the spread within each education group that is about twice as big for men. The propensity to work part time falls with the length of education for women, starting at 51% for the women with only lower secondary education or less and ending with 24% for the women in the group with highest education, but it is stable at 7% across all education groups for men. 65% of all the women in our sample work in the public sector in 2002, in the group of women with lower degree higher education the share is as high as 78%. The share of men working in the public sector in 2002 is about 50% in the groups with higher education, and about 27% in the groups with only secondary education.

84% in our sample of women who had children after 1993 took paid maternity leave with their first child. There is no variation in this share across education group. The

Table 1: Descriptive statistics.

	All		Secondary education				Higher education			
			Lower		Upper		Lower deg.		Higher deg.	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Women: Observations in 2002										
Hourly wage (1998 NOK)	138.17	(39.05)	123.69	(31.00)	129.98	(34.75)	147.86	(37.29)	180.82	(58.99)
Number of children	1.63	(1.04)	1.82	(0.91)	1.53	(1.00)	1.59	(1.12)	1.40	(1.12)
Age of first child	8.77	(8.21)	12.66	(7.54)	7.52	(7.57)	7.32	(8.19)	4.86	(7.19)
Age	37.14	(7.52)	40.52	(6.71)	35.55	(7.10)	36.08	(7.65)	34.75	(6.62)
Experience (years)	15.52	(7.48)	18.29	(7.86)	15.13	(6.68)	14.32	(7.21)	11.73	(6.43)
Cumulated parental leave (years)	0.63	(0.75)	0.49	(0.67)	0.70	(0.76)	0.68	(0.78)	0.71	(0.80)
Part time	0.42	(0.49)	0.51	(0.50)	0.42	(0.49)	0.38	(0.49)	0.24	(0.43)
Public sector	0.65	(0.48)	0.61	(0.49)	0.49	(0.50)	0.78	(0.41)	0.63	(0.48)
Women: Fixed variables, sample who had children after 1993										
Takes leave with first child	0.84	(0.37)	0.83	(0.38)	0.85	(0.36)	0.83	(0.37)	0.84	(0.37)
First child leave (years)	0.70	(0.42)	0.65	(0.40)	0.70	(0.41)	0.72	(0.44)	0.73	(0.45)
Full time before children	0.83	(0.38)	0.75	(0.43)	0.81	(0.39)	0.85	(0.36)	0.93	(0.25)
Public sector before children	0.60	(0.49)	0.49	(0.50)	0.43	(0.50)	0.73	(0.44)	0.61	(0.49)
N	243780		69085		60003		103130		11562	
Men: Observations in 2002										
Hourly wage (1998 NOK)	168.82	(74.77)	146.93	(62.12)	164.21	(70.79)	187.38	(77.24)	225.89	(89.63)
Number of children	1.77	(1.09)	1.92	(1.03)	1.65	(1.07)	1.72	(1.13)	1.84	(1.16)
Age of first child	10.76	(9.50)	13.72	(9.24)	8.93	(9.01)	9.27	(9.43)	10.01	(9.61)
Age	40.29	(8.61)	42.90	(8.10)	38.22	(8.45)	39.34	(8.57)	40.28	(8.60)
Experience (years)	19.70	(8.98)	22.71	(9.44)	18.59	(8.17)	17.65	(8.36)	17.48	(8.37)
Cumulated parental leave (years)	0.04	(0.11)	0.03	(0.08)	0.04	(0.10)	0.05	(0.12)	0.06	(0.15)
Part time	0.07	(0.25)	0.07	(0.26)	0.06	(0.23)	0.07	(0.26)	0.07	(0.26)
Public sector	0.35	(0.48)	0.27	(0.44)	0.26	(0.44)	0.52	(0.50)	0.48	(0.50)
Men: Fixed variables, sample who had children after 1993										
Takes leave with first child	0.58	(0.49)	0.50	(0.50)	0.59	(0.49)	0.62	(0.49)	0.64	(0.48)
First child leave (years)	0.05	(0.08)	0.04	(0.07)	0.04	(0.07)	0.05	(0.09)	0.07	(0.10)
Full time before children	0.92	(0.27)	0.91	(0.28)	0.94	(0.24)	0.90	(0.30)	0.95	(0.22)
Public sector before children	0.32	(0.47)	0.22	(0.41)	0.22	(0.42)	0.50	(0.50)	0.43	(0.50)
N	284900		99085		90763		73696		21356	

Note: Sample is women and men who had their first child before the end of 2007 and who are registered with employment in Statistics Norway's Wage statistic. Working less than 30 hours per week is classified as part time employment, working 30 hours or more is classified as fulltime.

corresponding number is 58% in the sample of all men who had children after 1993, and here the share is positively linked to the length of education.

3.4.1 Outcome variable: Hourly wage

Our measure of hourly wages is constructed using information on contracted hours and monthly wages in Statistics Norway's "Wage statistic" ("Lønnsstatistikken"). The Wage statistic is based on employer reports for a sample of Norwegian enterprizes on all employees by the 1st of October. Every year all public enterprizes and all private enterprizes with more than a certain number[13] of employees are included, for the remaining private sector a 50% sample of medium size enterprizes and a 20% sample of small enterprizes is drawn every year.[14] On average, the Wage statistic covers about 80% of Norwegian employees (100% of the public sector employees and 70% of the private sector employees) every year.

Contracted hours are given either in numbers or in percentages. Typically, public sector enterprizes report hours in percentages and private sector enterprizes report a number of hours per week. When reported in percentages, we use 100% = 37.5 hours per week.[15]

For every employment observation we calculate hourly wages by dividing contracted monthly wages - multiplied by $12 \times 7/365$ - by contracted hours per week. For individual's who are registered with more than one employment in a given year, we choose as the hourly wage from the employment where the individual works the most contracted hours (and in case of a tie, where he or she gets the most contracted wages) to represent the hourly wage of the individual that year.

The resulting panel is unbalanced, both due to the sampling of medium size enterprizes, and because people may move in and out of employment covered by the Wage statistic. A missing observation for a given year may mean either that the person in question does not work that year, that she is self-employed, or that she is employed in an enterprize that is not included in that year's sample.[16]

Using other official registries we find that about 20% of women and 7% of men who have a missing wage observation when their first child is 3 years old have earnings below the *basic amount* (G) of the Norwegian social security system[17], meaning that they are

[13] The number varies with industry and year.

[14] Employment in agriculture, hunting and forestry is left out. So are enterprizes with 3 or less employees.

[15] We have replaced the contracted number of hours by one tenth of the original number if it exceeds 70 hours per week, as the distribution of hours above this threshold peaks at typical numbers of hours times ten (f.i., there are peaks at 150, 175, 350, 355 and 375 hours per week).

[16] In our measure of hourly wages we leave all these observations as missing. The resulting estimates are therefore effects on the intensive margin.

[17] *G* ("Folketrygdens grunnbeløp") is adjusted yearly (or more often) in accordance with changes in the general income level. From January 1 2010, G is NOK 72 881 (apprioximately USD 12 500).

marginally or not employed at all that year. About 50% of the women and 75% of the men are registered as employed. Some of the remaining missing observations may indicate self-employment.

As employment status is influenced by parenthood, our results may be biased if the missing observations cover up hourly wages that are systematically different from the ones we observe. Missing observations due to time out of the labor force will most likely bias our results towards zero, as the true hourly wage for those not working at all could hardly be systematically higher than hourly wages for those working. It is less clear whether the missing observations on self-employment or non-sampled employment hide hourly wages that are higher or lower on average than the wages we observe.

3.4.2 Explanatory variables

The information on birth year, education and the linking of parents to their children comes from Statistics Norway demography, family and education registers.

In Section 3.6, we include four "bad controls" in order to see to what extent they explain the parenthood wage penalties. In Section 3.7 we divide the sample into different subsamples based on these four variables. *Experience* is constructed counting the cumulative number of years the individual is registered with occupational income (the variable "wyrkinnt" from Statistics Norways "Income registry" ("Registerbasert inntektsfil")) above 1G (see footnote 17). *Parental leave* counts the cumulative number of days the individual has been registered with paid parental leave (the variable "erdag" from Statistics Norway's birth payment register ("Fødselspengeregisteret")). Third, the variable *Part time* used in Section 3.6 is constructed as a dummy variable equal to one if the individual is registered as working 30 hours per week or less, zero if the registered number of weekly hours is more than 30. In Section 3.7, we split the sample according to whether the individual was registered with full time or part time work the year when they had their first child. Because we need information going back to 1993, we use the variable "forv_arb" from Statistics Norway's Employer/Employee register ("Arbeidstaker/arbeidsgiver-registeret" ("Areg")), which takes one of three values, according to whether the individual works 4-19, 20-29 and 30 or more hours per week, respectively. We include those who are registered with 30 or more hours per week in our "Full time" sample, those who are registered with 4-29 hours per week in our "Part time" sample. Lastly, the *Public sector* variable used in Section 3.6 is based on whether the individual is registered with a public sector employment code in the Wage statistic ("stilling" is non-missing). When we split the sample according to which sector individuals worked in the year they became parents, we again use information from the Employer/Employee register, going back to 1993.[18]

[18] Sector of employment is not directly observable in the Employer/Employee register. We thank Ola Lotherington Vestad for sharing his method for identifying public sector enterprizes in these data.

3.4.3 Sample

We study the years 1997 to 2007. Our sample consists of individuals born between 1952 (1947 if they are men) and 1990, who had their first child when they were no younger than 24 and no older than 40 (45) and who were observed to have at least one child by the end of 2007. We exclude parents who at one point experience multiple births, such as twins or triplets. For any given year, our sample consists of individuals between 21 and 50 (55) years of age.

3.5 Baseline results

The two panels of Figure 2 show the plotted age-wage profiles for women and men, sorted into different groups according to the age at which they have their first child. The vertical lines in the figure mark where each group start having children. Especially in the panel for women, it is clear that wages grow at about the same pace before they have children. When the child is born, wages flatten out and continue on a different path (what Wilde et al. (2010) call the "mommy track"). The picture is less clear for men. For some of the age groups (the < 25, 25-27 and 31-33 in particular) wages start lagging behind around the time when they have children, but there is no sign of a distinct "daddy track". The childless men and women on average have lower wages at the outset. For men they stay in the lower end throughout, reflecting a positive selection on wages of men into fatherhood. Due to the bifurcation of the mommy track the childless women end up earning more on average than the women who have children before they are 28 years old.

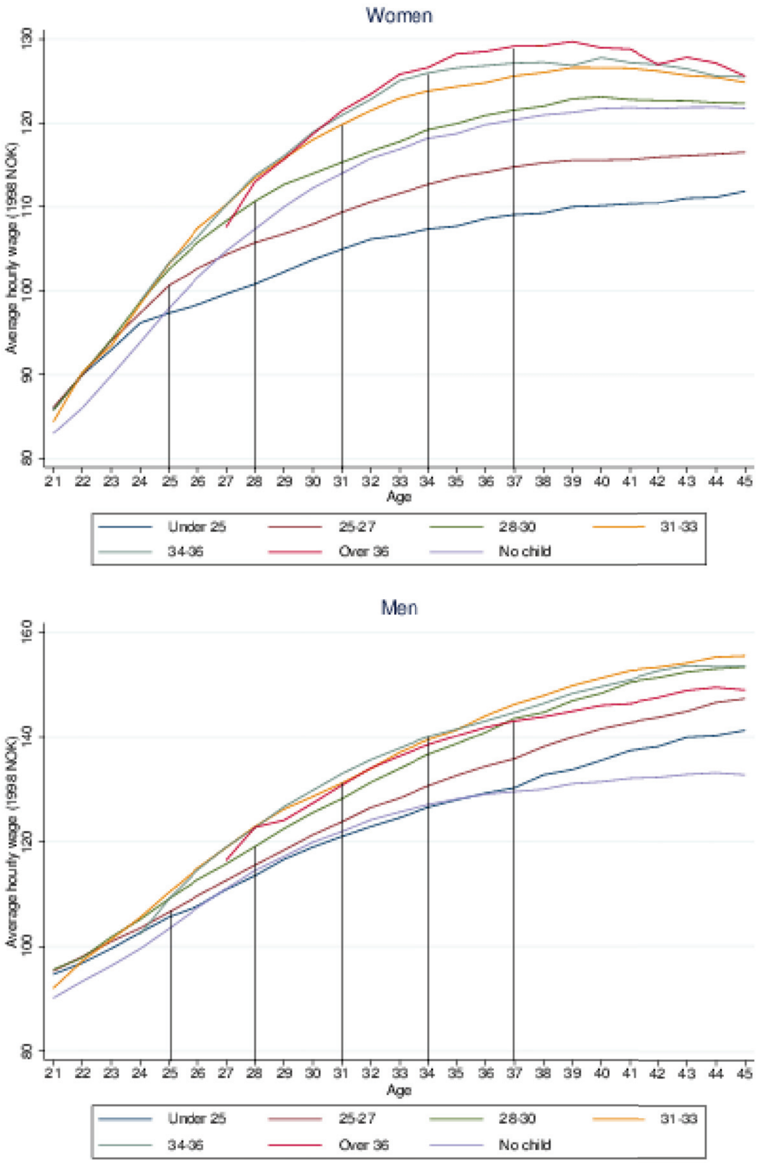
3.5.1 The impact of children on women's hourly wages

Table 2 shows the results from regression on Equation 1 for our sample of women. On average, having children reduces women's wages by 2.3%. The effect of having children is strongest for the highest education group, and weakest for the lowest education group. During the first years after having children wages are reduced by 4.6% for those who have a higher university degree and by 1.4% for women who do not have more than lower secondary education.

The effect does not wear off as the first child grows older. For the highest education category the effect is stable over time, meaning that after the first dip wages grow at the same rate as for those who have not yet had children. This corresponds to the scenario of the uppermost panel in Figure 1. For the three other categories, the effect gets stronger with time. This means that not only are wages reduced after the child is born, but they also continue to grow more slowly - as in the lowermost panel of Figure 1.

Relative to estimates on U.S. data, we find smaller wage penalties for Norwegian women. (Waldfogel, 1997; Budig and England, 2001; Anderson et al., 2002) all find

Figure 2: Age-wage profile by timing of first child



Note: Plot of age and mean wage of different timing-groups. Points are linearly connected. Mean wages are calculated at each age, adjusted for real wage growth and reported in 1998 prices.

Table 2: The effect of children on women's hourly wage.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
First child					
Age 1-5	-0.023***	-0.014***	-0.039***	-0.024***	-0.046***
	(0.00088)	(0.0021)	(0.0017)	(0.0012)	(0.0038)
Age 6-10	-0.031***	-0.017***	-0.048***	-0.032***	-0.047***
	(0.0012)	(0.0027)	(0.0024)	(0.0017)	(0.0055)
Age 11-15	-0.037***	-0.019***	-0.052***	-0.040***	-0.043***
	(0.0015)	(0.0031)	(0.0029)	(0.0021)	(0.0071)
Age >15	-0.040***	-0.019***	-0.050***	-0.043***	-0.041***
	(0.0017)	(0.0034)	(0.0033)	(0.0024)	(0.0088)
Second child					
Age 1-5	-0.012***	-0.00051	-0.020***	-0.019***	-0.037***
	(0.00079)	(0.0018)	(0.0015)	(0.0011)	(0.0037)
Age 6-10	-0.015***	-0.0047**	-0.026***	-0.025***	-0.035***
	(0.0011)	(0.0023)	(0.0021)	(0.0015)	(0.0055)
Age 11-15	-0.017***	-0.0074***	-0.027***	-0.029***	-0.037***
	(0.0013)	(0.0026)	(0.0026)	(0.0019)	(0.0073)
Age >15	-0.016***	-0.0065**	-0.022***	-0.031***	-0.038***
	(0.0015)	(0.0029)	(0.0031)	(0.0023)	(0.0094)
Third child					
Age 1-5	-0.0069***	0.0013	-0.016***	-0.014***	-0.039***
	(0.0011)	(0.0025)	(0.0022)	(0.0014)	(0.0049)
Age 6-10	-0.0084***	-0.0035	-0.020***	-0.023***	-0.043***
	(0.0014)	(0.0029)	(0.0029)	(0.0018)	(0.0070)
Age 11-15	-0.0061***	-0.0050	-0.022***	-0.027***	-0.051***
	(0.0016)	(0.0033)	(0.0036)	(0.0022)	(0.0095)
Age >15	-0.0027	-0.0066*	-0.0076*	-0.029***	-0.048***
	(0.0019)	(0.0037)	(0.0046)	(0.0027)	(0.014)
Observations	2426818	672269	610888	1024431	119230

Note: Each column provides FE estimates from a regression based on Equation 1. Year dummies, a polynomial in age and individual fixed effects are included in all specifications. So are controls for pregnancy, having a baby younger than one year and for having more than 3 children. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

estimated wage penalties for the first child of 4 to 6% with fixed effects estimation pooling all education groups, i.e. a similar specification to the one reported in column (1) of Table 2. Our estimate is around half the size. Wilde et al. (2010) use the method most similar to ours, by including only women who are observed to become mothers in the time window and by estimating the wage penalty separately for different skill groups. They find a negative wage effect of children of 7% for low skilled women and of 10% for high skilled women during the first four years after first birth. They find that the effect is increasing substantially in the years following, implying a much greater impact of children also on wage growth than the one we find for Norway.

Further children

Our empirical strategy, as described in Section 3.3.1, is directed at identifying the causal effect of the first child only. As our sample is restricted to parents observed to have at least one child, bias may result from adding controls for further children. We have carried out several alternative estimations, such as not controlling for subsequent children and restricting the sample to parents who are observed eventually to have at least two or at least three children, and the estimates of the effect of the first child are robust to them all.[19]

The estimated impact of the second and third child must nevertheless be interpreted with caution (although they do not change substantially when limiting the sample to individuals observed to have at least two and three children, respectively). There is an additional negative effect on wages of having a second and a third child for women. The effect is slightly smaller than the effect of the first child, but the patterns prevail: The effect is stronger for higher education groups, and it does not wear off with time.

The same pattern is found on U.S. data; the motherhood wage penalty is double (Anderson et al., 2002), or three times as large (Waldfogel, 1997; Budig and England, 2001) for two children or more. Wilde et al. (2010) also include a variable for number of additional children, and find each additional child has about half as large an impact as the first child.

3.5.2 The impact of children on men's hourly wages

Table 3 gives the results on the wage penalties to fatherhood. In our whole sample of men, irrespective of education level, the average effect of having children is not statistically significant at conventional levels. However, when we estimate the effect on wages separately for the four education groups we find statistically significant negative effects in all groups except for those who have no more than lower secondary education, where there is a positive and not statistically significant effect at .1%. For the other groups,

[19]Tables are available from the authors upon request.

Table 3: The effect of children on men's hourly wage.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
First child					
Age 1-5	0.00091	0.0014	-0.0059***	-0.0041**	-0.0051
	(0.0011)	(0.0021)	(0.0017)	(0.0019)	(0.0038)
Age 6-10	-0.0045***	0.0024	-0.0088***	-0.012***	-0.0048
	(0.0014)	(0.0027)	(0.0024)	(0.0026)	(0.0052)
Age 11-15	-0.0087***	0.00057	-0.0088***	-0.015***	-0.0058
	(0.0017)	(0.0031)	(0.0029)	(0.0032)	(0.0063)
Age >15	-0.011***	-0.00053	-0.0075**	-0.016***	-0.0095
	(0.0019)	(0.0035)	(0.0034)	(0.0036)	(0.0074)
Second child					
Age 1-5	0.0094***	0.011***	0.0019	0.00041	0.00083
	(0.00098)	(0.0019)	(0.0016)	(0.0017)	(0.0036)
Age 6-10	0.012***	0.012***	0.0033	0.00037	0.0057
	(0.0013)	(0.0024)	(0.0022)	(0.0024)	(0.0047)
Age 11-15	0.015***	0.016***	0.0028	0.000059	0.014**
	(0.0016)	(0.0028)	(0.0027)	(0.0029)	(0.0059)
Age >15	0.017***	0.017***	0.0041	0.0012	0.014**
	(0.0018)	(0.0031)	(0.0033)	(0.0034)	(0.0070)
Third child					
Age 1-5	0.010***	0.0092***	0.0073***	0.0021	-0.0013
	(0.0013)	(0.0025)	(0.0022)	(0.0023)	(0.0046)
Age 6-10	0.014***	0.010***	0.0085***	0.0036	-0.0011
	(0.0016)	(0.0030)	(0.0029)	(0.0030)	(0.0058)
Age 11-15	0.019***	0.015***	0.010***	0.0059*	-0.0013
	(0.0019)	(0.0034)	(0.0035)	(0.0035)	(0.0070)
Age >15	0.023***	0.020***	0.015***	0.0047	-0.011
	(0.0022)	(0.0039)	(0.0043)	(0.0040)	(0.0081)
Observations	2904039	997512	931936	754604	219987

Note: Each column provides FE estimates from a regression based on Equation 1. Year dummies, a polynomial in age and individual fixed effects are included in all specifications. So are controls for pregnancy, having a baby younger than one year and for having more than 3 children. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

having children causes a .4 to .6% decrease in wages during the first years after the child is born. For men in the two middle education groups, the effect grows significantly as the child gets older.

The statistically and economically significant wage penalty to fatherhood that we find here contrasts the findings of other studies on U.S. data (Lundberg and Rose, 2000, 2002; Simonsen and Skipper, 2008). Most of these studies include all men. Wilde et al. (2010) restrict their sample to those who become fathers and they also find small negative effects on wages of becoming a father. When we include men who are not observed to become fathers by 2008 in our sample, we find a much stronger positive association between having children and men's wages in the whole sample of men (see Table 13 in the Appendix). The pattern also applies in the different subgroups according to level of education, but the difference between including all men and including only eventual fathers does not matter as much here, except for in the group with lower secondary education or less. It thus seems that the main *methodological* reason for our contrasting findings on the wage penalty to fatherhood is the splitting into subsamples based on level of education, although restricting the sample to fathers-to-be only also plays a role. Apart from methodology, clearly the difference in institutional setting - both at the formal and at the informal level - between Norway and the U.S. may also matter for the diverging findings.

Further children

In Table 3 we see that having a second and a third child is positively associated with the level of men's wages. As for women, the warning about a causal interpretation of these estimates applies here - even to a larger extent. When we restrict the sample of men to those who are eventually observed to have at least two and at least three children, the estimated impact of the second and third child respectively becomes negative.[20]

3.6 Explaining the wage penalties to parenthood.

We investigate the determinants of the motherhood wage penalty in two different ways. In this section, we are concerned with the question of to what extent changes in observable circumstances explain the observed wage effect of children. The observables we have in mind are work experience, parental leave, part time work and sector of employment. These variables are explained more in detail in Section 3.4.2.

The results are displayed in Table 4. Because reliable data for parental leave exist only from 1993 onwards, the measure of paid parental leave can be constructed in the same way for all individuals only if they had their first child in 1993 or later. The sample is therefore restricted in this way. This means that we observe parents' wages until the first

[20]The baseline estimation of the wage penalty to fatherhood for the first child is however robust to having no controls for further children at all. Tables are available from the authors upon request.

Table 4: The effect of children on women's hourly wage. Including controls for experience, parental leave, part time and sector of employment.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline					
First child					
Age 1-5	-0.030***	-0.019***	-0.039***	-0.025***	-0.044***
	(0.00096)	(0.0024)	(0.0018)	(0.0013)	(0.0040)
Age 6-10	-0.044***	-0.024***	-0.046***	-0.033***	-0.045***
	(0.0014)	(0.0035)	(0.0027)	(0.0020)	(0.0060)
Age 11-13	-0.052***	-0.027***	-0.045***	-0.039***	-0.036***
	(0.0020)	(0.0046)	(0.0038)	(0.0029)	(0.0094)
Observations	1232595	207023	337515	601793	86264
Panel B: Including explanatory variables					
First child					
Age 1-5	-0.033***	-0.022***	-0.044***	-0.030***	-0.041***
	(0.0012)	(0.0032)	(0.0025)	(0.0016)	(0.0050)
Age 6-10	-0.047***	-0.027***	-0.052***	-0.038***	-0.041***
	(0.0016)	(0.0041)	(0.0032)	(0.0022)	(0.0067)
Age 11-13	-0.055***	-0.031***	-0.051***	-0.044***	-0.033***
	(0.0022)	(0.0051)	(0.0042)	(0.0031)	(0.0099)
Experience	0.0036***	0.0047***	0.0052***	0.0019*	0.0077**
	(0.00070)	(0.0014)	(0.0013)	(0.0011)	(0.0037)
Parental leave	-0.0021**	-0.0037	-0.00075	0.00063	-0.0037
	(0.00089)	(0.0025)	(0.0019)	(0.0011)	(0.0034)
Part time	0.017***	0.027***	0.025***	0.017***	-0.00052
	(0.00046)	(0.0012)	(0.00095)	(0.00060)	(0.0019)
Public sector	0.0045***	-0.0077***	-0.0063***	0.022***	-0.0051*
	(0.00065)	(0.0015)	(0.0011)	(0.0010)	(0.0027)
Observations	1232595	207023	337515	601793	86264

Note: Each column in each panel provides FE estimates from a regression based on Equation 1 for the sample of individuals who had their first child in 1994 or later. Year dummies, a polynomial in age, individual fixed effects and a set of dummies for having a second and a third child at various ages are included in all specifications. So are controls for pregnancy, having a baby younger than one year and for having more than 3 children. In the lower panel additional controls are included. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

child is 13 years old, at most, in 2007. In Panel A are the baseline results from estimating Equation 1 for this sample. Results are very similar to those for the whole sample, given in Table 2. In Panel B, the potential explanatory variables are included in the regression.

Comparing the two panels of Table 4, we see that inclusion of these variables does not significantly alter the coefficients. If anything, the estimated impact of children is larger when they are included. The results when we include only one variable at the time (given in Table 14 in the Appendix) show that adding measures for experience, maternity leave and sector of work does not alter the estimated impact of having children on women's wages. However, when a dummy for working less than 30 hours per week is added, the estimated wage penalty becomes greater. It is thus the case that working part time, in itself positively associated with hourly wage in all education groups (except in the highest one where there is no significant association), is also positively correlated with having children. When the part time dummy is omitted from the analysis, the effect of children on wages is smaller because switching to part time work ameliorates the wage penalty to motherhood.

The economically insignificant role of experience, part time work and parental leave in explaining the motherhood wage penalties are surprising, as these variables are found to explain a large part of the motherhood wage penalty in other countries (Waldfogel, 1997; Lundberg and Rose, 2000; Budig and England, 2001; Datta Gupta and Smith, 2002; Anderson et al., 2002; Wilde et al., 2010). One possible explanations for this is that policies of job protection during child related absence from work have an effect. A less optimistic explanation is that career breaks around birth are already accounted for in the wage offer that the woman gets even before pregnancy. This is the reasoning in Albrecht et al. (1999), who do not find a negative effect of child related career breaks for Swedish women. It could also be that women have already chosen jobs where the negative effects of a career break are small.

In themselves, experience and part time work are generally positively associated with the level of wages, whereas maternity leave seems to matter very little - if anything at all - for wages. Public sector work is negatively associated with wages in all education categories except for the one with lower degree higher education, where the association is strong and positive. If women choose to work in the public sector because they plan to have children (and think it is easier to combine with a public sector job), the true effect of having children on women's wages is larger than our estimates, as some of the cost is taken even before birth.

In Table 5 the corresponding results are given for men, for the sample who had their first child in 1993 or later. Panel A shows that in this sample the average effect of children on wages for all men, irrespective of level of education, is negative, at .6%. This sample thus differs significantly from the sample used in Section 3.5. This is reflected in a stronger wage penalty estimated for the two middle education groups, at .8 and .6% respectively

Table 5: The effect of children on men's hourly wage. Including controls for experience, parental leave, part time and sector of employment.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline					
First child					
Age 1-5	-0.0062***	-0.00041	-0.0076***	-0.0055***	-0.0022
	(0.0012)	(0.0023)	(0.0019)	(0.0021)	(0.0041)
Age 6-10	-0.016***	0.00068	-0.012***	-0.012***	0.0028
	(0.0017)	(0.0034)	(0.0028)	(0.0031)	(0.0059)
Age 11-13	-0.024***	-0.00023	-0.016***	-0.016***	0.014
	(0.0025)	(0.0047)	(0.0041)	(0.0046)	(0.0087)
Observations	1319323	321843	494896	393653	108931
Panel B: Including explanatory variables					
First child					
Age 1-5	-0.0049***	0.00046	-0.0039**	-0.0021	0.0034
	(0.0012)	(0.0024)	(0.0020)	(0.0021)	(0.0042)
Age 6-10	-0.015***	0.0016	-0.0085***	-0.0089***	0.0077
	(0.0018)	(0.0034)	(0.0029)	(0.0031)	(0.0060)
Age 11-13	-0.022***	0.00065	-0.013***	-0.013***	0.019**
	(0.0025)	(0.0047)	(0.0041)	(0.0046)	(0.0088)
Experience	-0.0080***	-0.0011	-0.0013	0.0052**	0.0038
	(0.0015)	(0.0028)	(0.0030)	(0.0024)	(0.0043)
Parental leave	-0.013***	-0.010	-0.047***	-0.038***	-0.046***
	(0.0028)	(0.0068)	(0.0054)	(0.0044)	(0.0073)
Part time	-0.00062	0.024***	0.026***	-0.019***	-0.047***
	(0.0014)	(0.0028)	(0.0030)	(0.0020)	(0.0046)
Public sector	-0.0065***	-0.011***	-0.0034***	0.012***	-0.015***
	(0.00070)	(0.0017)	(0.0012)	(0.0012)	(0.0023)
Observations	1319323	321843	494896	393653	108931

Note: Each column in each panel provides FE estimates from a regression based on Equation 1 for the sample of individuals who had their first child in 1994 or later. Year dummies, a polynomial in age, individual fixed effects and a set of dummies for having a second and a third child at various ages are included in all specifications. So are controls for pregnancy, having a baby younger than one year and for having more than 3 children. In the lower panel additional controls are included. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

(columns (3) and (4)).

When we include the potentially explanatory variables in the regression the impact is significant, although 79% of the effect estimated for the whole sample is left unexplained. The coefficient on having a first child aged between 1 and 5 years is no longer significant for the group with higher education, lower degree (column (4)), and for the group with upper secondary education it is cut in half. Also the persistence of the effect, reflected in the coefficients on having a first child aged 6 to 10 years and 11 to 14 years, is reduced, although not by as much.

Including each of these variables separately shows that the wage penalty to fatherhood does not at all covary with experience, part time work or sector of employment (the results are displayed in Table 15 in the Appendix). It is the inclusion of parental leave that matters for the estimated effect on men's wages of having children. In the two groups where having children does have a statistically significant impact on men's wages, the inclusion of paternity leave as an explanatory variable explains a substantial part of the effect.

The variation in parental leave in itself explains a great deal of men's hourly wages. For the three highest education groups (columns (3) to (5)), one year of parental leave is associated with a 4 to 5% reduction in wages - more than the whole wage penalty for women, who typically take somewhat less than a year of leave. We are not able to determine to what extent this is due to the adverse effect of paternity leave on human capital accumulation or whether the length of paternity leave reflects men's propensity to spend more effort at home relative to market work, thereby reducing wages. The finding that parental leave is more strongly associated with the wage effects of parenthood for men than for women is in accordance with Albrecht et al. (1999)'s finding in their study on Swedish data. They find that career breaks around birth are associated with negative wage effects only for men, not for women. Their interpretation is that the negative wage effect stems from the employer offering lower wages to women even before they have children because they expect women to have a career break around birth. Employers do not expect the same from men, and parental leave is therefore a stronger signal of the strength of men's commitment to market work. Another possible explanation that would give the same empirical pattern, however, is that men to a larger extent than women choose jobs where a career break has a larger effect.

Part time work is associated with higher wages for those with only secondary education (columns (2) and (3)) and with lower wages for those with higher education (columns (4) and (5)). Public sector work is generally associated with having lower wages, except for the in the group with lower degree higher education - just as is the case for women.

3.7 Heterogeneous effects

In this section we investigate whether the wage penalty is stronger for some groups than for others. We do this by running separate regressions on different subgroups; on those who take relatively longer or shorter parental leave, on those who work full time and part time, and on those who work in the public or in the private sector. Individuals are sorted into groups based on the observed length of parental leave taken with the first child, or according to sector or working time category the year before their first child is born. As the variables we condition on are available from 1993 onwards, we limit the sample to individuals having their first child after 1993 (like in Section 3.6).

3.7.1 Parental leave

Probably the most direct labor market effect of having children is the period of parental leave.[21]. In Tables 6 and 7, we investigate whether parenthood wage effects are stronger for those who take relatively longer leave. We have divided the samples in four: Those who take no, little, middle and long leave. The division into "little", "middle" and "long" is based on the assigned percentile in the distribution of all parents of the same sex with children born in the same quarter of the year. The distribution is divided in three. For men, though, there is very little spread in the number of leave days taken, and the middle group - containing only 2140 individuals (.16% of the sample) - has therefore been excluded from the analysis.

Among the parents who are not registered with taking any paid parental leave are both individuals who have have not earned the right to paid parental leave and individuals who may have earned the right but do not use it. It is very uncommon for women not to use the right to maternity leave, hence the group of women taking no leave can generally be thought of as consisting of women who did not earn the right. For men, it is much more common not to take the paid leave granted them. Also, men's right to wage compensated paternity leave does not only depend on their own earned right but also on the child's mother having earned the right. Hence the group of men taking no leave is more mixed, consisting of men who did not earn the right, men whose spouse did not earn the right, and men who have the right to paid parental leave but do not use it.

The results for women are given in Table 6. On average, there is not much variation in the immediate effect of having children between the four groups, and though there is some variation within each education group according to the relative length of maternity leave taken with the first child, there is no clear pattern of statistically significant difference between the groups. In the group of women with the highest education (column (5)), taking longer leave, relative to taking no leave at all, is clearly associated with a greater

[21]Table 1 shows that in our sample of parents who had their first child in 1994 or later, women on average took .7 years of paid leave with their first child, and men .05 years.

Table 6: The effect of children on women's hourly wage. Subsample analysis according to relative length of parental leave.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
Panel A: No parental leave					
First child					
Age 1-5	-0.028***	-0.031***	-0.048***	-0.013**	-0.020
	(0.0039)	(0.0090)	(0.0080)	(0.0052)	(0.017)
Age 6-10	-0.030***	-0.030***	-0.037***	-0.012*	-0.045**
	(0.0051)	(0.011)	(0.011)	(0.0069)	(0.022)
Age 11-13	-0.029***	-0.028**	-0.025	-0.017*	-0.062*
	(0.0070)	(0.014)	(0.015)	(0.0098)	(0.032)
Observations	199415	39353	47206	98259	14597
Panel B: Short length parental leave					
First child					
Age 1-5	-0.023***	-0.012***	-0.031***	-0.023***	-0.041***
	(0.0018)	(0.0042)	(0.0032)	(0.0025)	(0.0073)
Age 6-10	-0.035***	-0.015**	-0.037***	-0.029***	-0.044***
	(0.0026)	(0.0061)	(0.0048)	(0.0038)	(0.011)
Age 11-13	-0.040***	-0.012	-0.036***	-0.032***	-0.035**
	(0.0037)	(0.0082)	(0.0066)	(0.0055)	(0.017)
Observations	347595	65467	100336	157854	23938
Panel C: Middle length parental leave					
First child					
Age 1-5	-0.030***	-0.015***	-0.045***	-0.026***	-0.049***
	(0.0018)	(0.0043)	(0.0033)	(0.0025)	(0.0085)
Age 6-10	-0.046***	-0.017***	-0.058***	-0.035***	-0.053***
	(0.0027)	(0.0063)	(0.0050)	(0.0039)	(0.013)
Age 11-13	-0.058***	-0.023***	-0.061***	-0.043***	-0.050**
	(0.0039)	(0.0083)	(0.0070)	(0.0056)	(0.020)
Observations	316632	57262	94689	146580	18101
Panel D: Long length parental leave					
First child					
Age 1-5	-0.025***	-0.022***	-0.042***	-0.018***	-0.049***
	(0.0021)	(0.0062)	(0.0043)	(0.0027)	(0.0076)
Age 6-10	-0.038***	-0.033***	-0.049***	-0.028***	-0.048***
	(0.0030)	(0.0084)	(0.0060)	(0.0039)	(0.011)
Age 11-13	-0.052***	-0.050***	-0.055***	-0.042***	-0.028*
	(0.0041)	(0.011)	(0.0083)	(0.0054)	(0.017)
Observations	368953	44941	95284	199100	29628

Note: Each column in each panel provides FE estimates from a regression based on Equation 1 for the sample of individuals who had their first child in 1994 or later. Year dummies, a polynomial in age, individual fixed effects and controls for further children, being pregnant and having a baby younger than one year are included in all specifications. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 7: The effect of children on men's hourly wage. Subsample analysis according to relative length of parental leave.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
Panel A: No parental leave					
First child					
Age 1-5	-0.016***	-0.0068*	-0.0088***	-0.013***	0.0021
	(0.0020)	(0.0035)	(0.0033)	(0.0038)	(0.0077)
Age 6-10	-0.028***	-0.0091*	-0.015***	-0.025***	0.015
	(0.0029)	(0.0049)	(0.0048)	(0.0054)	(0.011)
Age 11-13	-0.040***	-0.013**	-0.024***	-0.030***	0.019
	(0.0040)	(0.0068)	(0.0066)	(0.0078)	(0.015)
Observations	559026	166016	202312	151866	38832
Panel B: Short length parental leave					
First child					
Age 1-5	0.0020	0.0037	-0.0026	0.00083	0.00065
	(0.0016)	(0.0035)	(0.0026)	(0.0029)	(0.0062)
Age 6-10	-0.0057**	0.0068	-0.0057	-0.0031	0.00100
	(0.0024)	(0.0050)	(0.0038)	(0.0042)	(0.0089)
Age 11-13	-0.013***	0.0056	-0.0090	-0.0073	0.015
	(0.0035)	(0.0070)	(0.0055)	(0.0063)	(0.013)
Observations	628880	137075	253575	190723	47507
Panel C: Long length parental leave					
First child					
Age 1-5	-0.015***	-0.0020	-0.020***	-0.015***	-0.017**
	(0.0033)	(0.0085)	(0.0065)	(0.0048)	(0.0078)
Age 6-10	-0.020***	0.013	-0.021**	-0.020***	-0.016
	(0.0049)	(0.012)	(0.0094)	(0.0074)	(0.011)
Age 11-13	-0.018**	0.031*	-0.018	-0.025**	0.0029
	(0.0076)	(0.018)	(0.015)	(0.012)	(0.018)
Observations	129277	18414	37949	50460	22454

Note: Each column in each panel provides FE estimates from a regression based on Equation 1 for the sample of individuals who had their first child in 1994 or later. Year dummies, a polynomial in age, individual fixed effects and controls for further children, being pregnant and having a baby younger than one year are included in all specifications. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

wage penalty to having children the first four years after the wage compensated maternity leave period is over. For the two groups with no more than secondary education (columns (2) and (3)), the wage penalty during the first years is stronger for the group who does not take paid maternity leave, and then it is increasing in the relative length of the leave period. All in all, it does not seem that the wage penalty to motherhood is not very clearly linked to the length of maternity leave - as is also found by Albrecht et al. (1999).

For the women who are registered with middle and longer periods of paid maternity leave, the wage penalty grows significantly with time in all education groups but the highest one, expressing a pattern of increasing differences between actual wages and the reference path, as illustrated in the lowermost panel of Figure 1. Taking longer leave is thus associated with a larger negative effect both on wage levels and wage growth.

The results for men are given in Table 7. For men, there is a U-shaped relationship between the penalty and the propensity to take leave. The wage penalty to fatherhood is greatest for the group of men taking no leave and the group of men taking longer leave than what is usual. Again, this is in line with the findings of Albrecht et al. (1999).[22] The average wage penalty in these two groups of men is considerable, at about 1.5%.

3.7.2 Full time vs. part time employment

Having children is associated with a reduction in working hours both for men and women - but mainly for women (Cools and Strøm, 2012). As we saw in Section 3.6, changing working time status to part time is what correlates most with a motherhood wage penalty. Here we investigate whether wage penalties are smaller for those who already work part time before having children. However, if part time work is chosen in advance because the individual plans to have a child, this would cause an upward bias in our estimates of the effect of having children.

The results for women are given in Panel A and B in Table 11. On average the negative wage effects of having children are two to three times stronger for the women who were registered with full time employment the year before they had children, compared to the women who had part time employment before having children. Also, on average, there is a clear pattern of the wage penalty increasing over time for the group of full time working women, reaching a wage reduction of 6.2% when the first child turns 11 years old. In the very small group of women with the highest education who worked part time before having children, we see the first instance of a wage premium to having children, reaching 7.8% and significant at the 5% level by the time the child turns 11 years old.

Panel C and D in Table 11 give the corresponding results for men. Here, the difference between the two groups is even more striking. The men who worked full time before

[22] It is not necessary, however, to evoke the employer as a signal taker, finding more new information in men's parental leave behavior than in women's. It may also be that the group of men taking relatively longer leave is just a select group of male employees who give less priority to their careers after having children than men who take shorter paternity leave do.

Table 8: The effect of children on hourly wage. Subsample analysis according to being employed full time or part time the year before having children.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
Panel A: Full time, women					
First child					
Age 1-5	-0.035***	-0.020***	-0.043***	-0.029***	-0.050***
	(0.0011)	(0.0029)	(0.0021)	(0.0014)	(0.0043)
Age 6-10	-0.051***	-0.028***	-0.052***	-0.039***	-0.052***
	(0.0017)	(0.0043)	(0.0032)	(0.0022)	(0.0065)
Age 11-13	-0.062***	-0.032***	-0.054***	-0.048***	-0.040***
	(0.0024)	(0.0058)	(0.0045)	(0.0033)	(0.010)
Observations	887958	122553	242040	452112	71253
Panel B: Part time, women					
First child					
Age 1-5	-0.013***	-0.0081	-0.023***	-0.011***	0.020
	(0.0026)	(0.0053)	(0.0046)	(0.0040)	(0.018)
Age 6-10	-0.019***	-0.013*	-0.031***	-0.011*	0.037
	(0.0038)	(0.0075)	(0.0067)	(0.0058)	(0.025)
Age 11-13	-0.022***	-0.022**	-0.028***	-0.011	0.078**
	(0.0053)	(0.010)	(0.0094)	(0.0082)	(0.037)
Observations	186753	42881	57651	81316	4905
Panel C: Full time, men					
First child					
Age 1-5	-0.0088***	-0.0013	-0.0085***	-0.0079***	-0.0021
	(0.0013)	(0.0026)	(0.0020)	(0.0023)	(0.0044)
Age 6-10	-0.024***	-0.0044	-0.016***	-0.018***	0.0011
	(0.0019)	(0.0038)	(0.0030)	(0.0035)	(0.0064)
Age 11-13	-0.037***	-0.0094*	-0.021***	-0.027***	0.0090
	(0.0028)	(0.0054)	(0.0044)	(0.0052)	(0.0094)
Observations	1041909	232554	412447	306987	89921
Panel D: Part time, men					
First child					
Age 1-5	0.0100**	0.0036	0.0086	0.018**	-0.020
	(0.0048)	(0.0094)	(0.0094)	(0.0073)	(0.021)
Age 6-10	0.027***	0.023*	0.019	0.036***	0.012
	(0.0069)	(0.013)	(0.013)	(0.010)	(0.029)
Age 11-13	0.043***	0.027	0.022	0.065***	0.057
	(0.0099)	(0.020)	(0.019)	(0.015)	(0.043)
Observations	86209	22292	25031	34136	4750

Note: Each column in each panel provides FE estimates from a regression based on Equation 1 for the sample of individuals who had their first child in 1994 or later. Year dummies, a polynomial in age, individual fixed effects and controls for further children, being pregnant and having a baby younger than one year are included in all specifications. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

becoming fathers on average experience a wage penalty to fatherhood, at .9%. This is a 50% larger penalty than the .6% wage penalty estimated for the whole sample given in Table 5. The penalty grows significantly over time, like the case is for women who work full time before having children. The wage penalty estimated for the sample of full time working men is on average stronger than for the part time working women, especially as the first child grows older.

For men who worked part time before becoming fathers, on the other hand, there are statistically significant wage premia to fatherhood when looking at the whole sample (column (1) in Panel D) and in the group with higher education, lower degree. The premium is growing over time.

3.7.3 Sector of employment

Working in the public sector is seen as more "family-friendly" than the private sector because of less overtime, more centrally set wages (hence less prone to be influenced by having children) and generally lower risk of job loss. The cost is that wages in the public sector are generally lower than in the private sector. The variation in wages in the public sector is smaller than the variation in wages in the private sector - and potentially also the parenthood wage effects.

Looking at the results in Table 9, it is clear that the wage penalty to parenthood is larger in the private than in the public sector. For men, there is no wage penalty at all in the public sector, whereas there is a 1% wage reduction in the private sector for all education groups except for the one with no more than lower secondary education. For women, the wage penalty in the public sector is only slightly higher than the one men face in the private sector. In the private sector, the wage penalty to motherhood is three times higher on average than in the public sector. For women in both the public and private enterprizes and for men in the private sector, the wage penalty to parenthood grows over time.

3.8 Concluding remarks

In spite of the encouragement built into the labor market institutions of the Nordic countries to combine work and family, the two remain in conflict, as is pointed out by the wage penalties to parenthood documented in this paper. Like in most other countries, women bear the greater share of the cost of having children also in Norway.

The wage penalties to motherhood are substantial for the women who have more to lose in terms of a career by having children; the higher educated women, the women working full and in the private sector. Contrary to what other studies have found using U.S. data, the wage penalty to motherhood is not at all explained by time spent out of

Table 9: The effect of children on hourly wage. Subsample analysis according to being employed in the public or in the private sector the year before having children.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
Panel A: Public sector, women					
First child					
Age 1-5	-0.018***	-0.000098	-0.016***	-0.015***	-0.034***
	(0.0011)	(0.0031)	(0.0024)	(0.0014)	(0.0049)
Age 6-10	-0.030***	-0.0062	-0.027***	-0.022***	-0.029***
	(0.0017)	(0.0046)	(0.0037)	(0.0022)	(0.0074)
Age 11-13	-0.039***	-0.015**	-0.029***	-0.028***	-0.014
	(0.0025)	(0.0062)	(0.0053)	(0.0032)	(0.012)
Observations	647501	81813	129469	390330	45889
Panel B: Private sector, women					
First child					
Age 1-5	-0.056***	-0.035***	-0.060***	-0.056***	-0.065***
	(0.0019)	(0.0041)	(0.0029)	(0.0033)	(0.0073)
Age 6-10	-0.078***	-0.041***	-0.067***	-0.071***	-0.081***
	(0.0028)	(0.0059)	(0.0043)	(0.0049)	(0.011)
Age 11-13	-0.092***	-0.041***	-0.068***	-0.081***	-0.080***
	(0.0040)	(0.0080)	(0.0059)	(0.0071)	(0.017)
Observations	427210	83621	170222	143098	30269
Panel C: Public sector, men					
First child					
Age 1-5	0.0010	0.0062	0.00058	0.00020	0.0092
	(0.0018)	(0.0046)	(0.0036)	(0.0026)	(0.0061)
Age 6-10	-0.00061	0.0056	0.00015	0.00033	0.023***
	(0.0027)	(0.0069)	(0.0053)	(0.0038)	(0.0088)
Age 11-13	-0.0023	0.0024	0.0000059	-0.0025	0.040***
	(0.0040)	(0.0095)	(0.0075)	(0.0057)	(0.013)
Observations	357603	53382	94413	169848	39960
Panel D: Private sector, men					
First child					
Age 1-5	-0.012***	-0.0032	-0.011***	-0.0099***	-0.012**
	(0.0016)	(0.0029)	(0.0023)	(0.0035)	(0.0058)
Age 6-10	-0.031***	-0.0041	-0.019***	-0.025***	-0.014*
	(0.0024)	(0.0043)	(0.0035)	(0.0052)	(0.0084)
Age 11-13	-0.047***	-0.0084	-0.026***	-0.030***	-0.0051
	(0.0035)	(0.0062)	(0.0051)	(0.0078)	(0.013)
Observations	770515	201464	343065	171275	54711

Note: Each column in each panel provides FE estimates from a regression based on Equation 1 for the sample of individuals who had their first child in 1994 or later. Year dummies, a polynomial in age, individual fixed effects and controls for further children, being pregnant and having a baby younger than one year are included in all specifications. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

the labor force. *After* returning to work, mothers bear the cost of having children to a larger extent than fathers do.

Contrary to earlier studies, both on U.S. and Norwegian data, we find negative (though comparatively small) wage penalties also to fatherhood. This is consistent with men's increasing involvement in child rearing - hence experiencing a private cost in terms of lower wages. Parenthood is associated with a larger wage gap between men and women in most countries. This still applies to Norway, but to a lesser extent.

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Table 10: The effect of children on women's hourly wage. Including dummies for the years before having children.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
First child					
2 yrs pp	0.016***	0.0064***	0.00082	0.011***	0.0091***
	(0.00080)	(0.0021)	(0.0015)	(0.0011)	(0.0032)
Age 1-5	-0.014***	-0.012***	-0.040***	-0.017***	-0.042***
	(0.0012)	(0.0027)	(0.0022)	(0.0016)	(0.0053)
Age 6-10	-0.020***	-0.014***	-0.049***	-0.024***	-0.040***
	(0.0015)	(0.0033)	(0.0029)	(0.0021)	(0.0069)
Age 11-15	-0.025***	-0.016***	-0.054***	-0.030***	-0.036***
	(0.0017)	(0.0036)	(0.0033)	(0.0025)	(0.0084)
Age >15	-0.027***	-0.016***	-0.052***	-0.032***	-0.033***
	(0.0019)	(0.0039)	(0.0038)	(0.0028)	(0.0099)
Second child					
2 yrs pp	0.010***	0.0076***	0.0076***	0.0056***	0.0077**
	(0.00081)	(0.0021)	(0.0016)	(0.0011)	(0.0035)
Age 1-5	-0.0051***	0.0032	-0.017***	-0.014***	-0.031***
	(0.00097)	(0.0022)	(0.0018)	(0.0014)	(0.0046)
Age 6-10	-0.0072***	-0.00070	-0.023***	-0.019***	-0.029***
	(0.0012)	(0.0026)	(0.0024)	(0.0018)	(0.0063)
Age 11-15	-0.0075***	-0.0032	-0.024***	-0.022***	-0.030***
	(0.0014)	(0.0029)	(0.0028)	(0.0021)	(0.0080)
Age >15	-0.0062***	-0.0021	-0.019***	-0.023***	-0.030***
	(0.0017)	(0.0031)	(0.0034)	(0.0025)	(0.0100)
Third child					
2 yrs pp	0.0083***	0.0072**	0.0079***	0.0016	0.0093**
	(0.0011)	(0.0032)	(0.0025)	(0.0014)	(0.0047)
Age 1-5	-0.0031***	0.0043	-0.013***	-0.013***	-0.035***
	(0.0012)	(0.0029)	(0.0025)	(0.0016)	(0.0054)
Age 6-10	-0.0042***	-0.00033	-0.017***	-0.021***	-0.038***
	(0.0015)	(0.0033)	(0.0031)	(0.0020)	(0.0074)
Age 11-15	-0.0018	-0.0019	-0.019***	-0.025***	-0.046***
	(0.0017)	(0.0036)	(0.0038)	(0.0023)	(0.0098)
Age >15	0.0023	-0.0033	-0.0046	-0.026***	-0.043***
	(0.0020)	(0.0040)	(0.0047)	(0.0028)	(0.014)
Observations	2426818	672269	610888	1024431	119230

Note: Each column provides FE estimates from a regression based on Equation 1. Year dummies, a polynomial in age and individual fixed effects are included in all specifications. So are controls for pregnancy, having a baby younger than one year and for having more than 3 children. In addition three dummies are included for observations in the two years preceding the three first pregnancies. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 11: The effect of children on men's hourly wage. Including dummies for the years before having children.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
First child					
2 yrs pp	0.012***	0.0045**	0.0023	0.0060***	0.0012
	(0.00096)	(0.0020)	(0.0016)	(0.0017)	(0.0033)
Age 1-5	0.0067***	0.0028	-0.0050**	-0.00072	-0.0068
	(0.0014)	(0.0026)	(0.0022)	(0.0025)	(0.0050)
Age 6-10	0.0030*	0.0042	-0.0075***	-0.0072**	-0.0065
	(0.0017)	(0.0032)	(0.0029)	(0.0031)	(0.0063)
Age 11-15	-0.00019	0.0026	-0.0074**	-0.0097***	-0.0074
	(0.0020)	(0.0036)	(0.0034)	(0.0037)	(0.0074)
Age >15	-0.0020	0.0016	-0.0059	-0.011***	-0.011
	(0.0022)	(0.0039)	(0.0038)	(0.0041)	(0.0083)
Second child					
2 yrs pp	0.0077***	0.0057***	0.0024	0.0024	0.0064*
	(0.00099)	(0.0020)	(0.0016)	(0.0017)	(0.0036)
Age 1-5	0.014***	0.014***	0.0032	0.0023	0.0045
	(0.0012)	(0.0023)	(0.0020)	(0.0022)	(0.0045)
Age 6-10	0.018***	0.015***	0.0048*	0.0029	0.0096*
	(0.0015)	(0.0028)	(0.0025)	(0.0028)	(0.0055)
Age 11-15	0.021***	0.019***	0.0044	0.0029	0.017***
	(0.0017)	(0.0031)	(0.0030)	(0.0033)	(0.0066)
Age >15	0.023***	0.020***	0.0058*	0.0043	0.018**
	(0.0020)	(0.0034)	(0.0035)	(0.0037)	(0.0077)
Third child					
2 yrs pp	0.0038***	-0.0012	0.0013	0.0025	-0.00096
	(0.0014)	(0.0027)	(0.0025)	(0.0024)	(0.0044)
Age 1-5	0.012***	0.0090***	0.0078***	0.0032	-0.0014
	(0.0015)	(0.0028)	(0.0025)	(0.0026)	(0.0052)
Age 6-10	0.017***	0.010***	0.0092***	0.0049	-0.0011
	(0.0018)	(0.0033)	(0.0032)	(0.0032)	(0.0063)
Age 11-15	0.022***	0.015***	0.011***	0.0074**	-0.0013
	(0.0021)	(0.0037)	(0.0037)	(0.0038)	(0.0074)
Age >15	0.025***	0.020***	0.016***	0.0065	-0.011
	(0.0024)	(0.0041)	(0.0044)	(0.0042)	(0.0085)
Observations	2904039	997512	931936	754604	219987

Note: Each column provides FE estimates from a regression based on Equation 1. Year dummies, a polynomial in age and individual fixed effects are included in all specifications. So are controls for pregnancy, having a baby younger than one year and for having more than 3 children. In addition three dummies are included for observations in the two years preceding the three first pregnancies. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 12: The effect of children on women's hourly wage, including women who are not observed to have children in the sample.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
First child					
Age 1-5	-0.0098***	-0.017***	-0.038***	-0.024***	-0.045***
	(0.00078)	(0.0018)	(0.0015)	(0.0011)	(0.0037)
Age 6-10	-0.013***	-0.021***	-0.049***	-0.032***	-0.046***
	(0.0010)	(0.0022)	(0.0020)	(0.0016)	(0.0053)
Age 11-15	-0.016***	-0.024***	-0.055***	-0.039***	-0.043***
	(0.0012)	(0.0025)	(0.0024)	(0.0019)	(0.0068)
Age >15	-0.017***	-0.025***	-0.056***	-0.041***	-0.044***
	(0.0014)	(0.0026)	(0.0028)	(0.0022)	(0.0084)
Second child					
Age 1-5	-0.0098***	-0.0033**	-0.020***	-0.018***	-0.035***
	(0.00069)	(0.0015)	(0.0013)	(0.0010)	(0.0035)
Age 6-10	-0.012***	-0.0087***	-0.026***	-0.024***	-0.033***
	(0.00092)	(0.0018)	(0.0017)	(0.0014)	(0.0052)
Age 11-15	-0.014***	-0.012***	-0.029***	-0.028***	-0.035***
	(0.0011)	(0.0020)	(0.0021)	(0.0018)	(0.0070)
Age >15	-0.012***	-0.011***	-0.024***	-0.030***	-0.035***
	(0.0012)	(0.0022)	(0.0025)	(0.0021)	(0.0089)
Third child					
Age 1-5	-0.0086***	-0.0019	-0.013***	-0.015***	-0.035***
	(0.00084)	(0.0016)	(0.0016)	(0.0012)	(0.0046)
Age 6-10	-0.012***	-0.0067***	-0.018***	-0.024***	-0.038***
	(0.0011)	(0.0019)	(0.0021)	(0.0016)	(0.0066)
Age 11-15	-0.010***	-0.0070***	-0.017***	-0.026***	-0.042***
	(0.0012)	(0.0021)	(0.0026)	(0.0020)	(0.0089)
Age >15	-0.0088***	-0.0046**	-0.0061**	-0.028***	-0.045***
	(0.0014)	(0.0023)	(0.0031)	(0.0024)	(0.013)
Observations	3778746	1537899	918022	1194770	128055

Note: Each column provides FE estimates from a regression based on Equation 1. Year dummies, a polynomial in age and individual fixed effects are included in all specifications. So are controls for pregnancy, having a baby younger than one year and for having more than 3 children. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 13: The effect of children on men's hourly wage, including men who are not observed to have children in the sample.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
First child					
Age 1-5	0.0076***	0.0036*	-0.0043***	-0.0043**	-0.0049
	(0.00099)	(0.0019)	(0.0016)	(0.0018)	(0.0037)
Age 6-10	0.0036***	0.0048*	-0.0080***	-0.012***	-0.0044
	(0.0013)	(0.0024)	(0.0022)	(0.0024)	(0.0050)
Age 11-15	-0.00083	0.0026	-0.0096***	-0.016***	-0.0068
	(0.0016)	(0.0028)	(0.0027)	(0.0030)	(0.0061)
Age >15	-0.0036**	0.0018	-0.0091***	-0.019***	-0.012*
	(0.0018)	(0.0031)	(0.0031)	(0.0034)	(0.0071)
Second child					
Age 1-5	0.0094***	0.0090***	0.0018	0.00068	0.0024
	(0.00092)	(0.0017)	(0.0015)	(0.0017)	(0.0034)
Age 6-10	0.012***	0.011***	0.0023	-0.00086	0.0079*
	(0.0012)	(0.0022)	(0.0020)	(0.0023)	(0.0046)
Age 11-15	0.015***	0.014***	0.0022	-0.0014	0.015***
	(0.0015)	(0.0025)	(0.0025)	(0.0028)	(0.0057)
Age >15	0.016***	0.015***	0.0016	-0.00074	0.014**
	(0.0017)	(0.0028)	(0.0030)	(0.0032)	(0.0068)
Third child					
Age 1-5	0.0078***	0.0084***	0.0047**	0.00072	-0.00028
	(0.0011)	(0.0021)	(0.0019)	(0.0021)	(0.0043)
Age 6-10	0.011***	0.0095***	0.0060**	0.0018	0.0019
	(0.0014)	(0.0025)	(0.0025)	(0.0028)	(0.0055)
Age 11-15	0.015***	0.013***	0.0085***	0.0031	0.0032
	(0.0017)	(0.0029)	(0.0030)	(0.0033)	(0.0067)
Age >15	0.017***	0.017***	0.013***	0.0028	-0.0077
	(0.0019)	(0.0032)	(0.0036)	(0.0037)	(0.0077)
Observations	3649791	1413929	1148953	846634	240275

Note: Each column provides FE estimates from a regression based on Equation 1. Year dummies, a polynomial in age and individual fixed effects are included in all specifications. So are controls for pregnancy, having a baby younger than one year and for having more than 3 children. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Table 14: The effect of children on women's hourly wage. Including controls for experience, parental leave, part time and sector of employment in separate regressions.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
First child					
Age 1-5	-0.030***	-0.018***	-0.038***	-0.025***	-0.044***
	(0.00096)	(0.0024)	(0.0018)	(0.0013)	(0.0040)
Age 6-10	-0.044***	-0.023***	-0.045***	-0.033***	-0.044***
	(0.0014)	(0.0035)	(0.0027)	(0.0020)	(0.0060)
Age 11-13	-0.052***	-0.027***	-0.045***	-0.039***	-0.036***
	(0.0020)	(0.0046)	(0.0038)	(0.0029)	(0.0094)
Experience	0.0033***	0.0042***	0.0052***	0.0013	0.0074**
	(0.00069)	(0.0014)	(0.0013)	(0.0010)	(0.0037)
Observations	1232595	207023	337515	601793	86264
First child					
Age 1-5	-0.028***	-0.017***	-0.039***	-0.024***	-0.042***
	(0.0012)	(0.0032)	(0.0024)	(0.0016)	(0.0049)
Age 6-10	-0.042***	-0.022***	-0.046***	-0.033***	-0.043***
	(0.0016)	(0.0041)	(0.0032)	(0.0022)	(0.0067)
Age 11-13	-0.050***	-0.025***	-0.045***	-0.039***	-0.034***
	(0.0022)	(0.0051)	(0.0042)	(0.0030)	(0.0099)
Parental leave	-0.0020**	-0.0024	0.00012	-0.00051	-0.0024
	(0.00088)	(0.0025)	(0.0019)	(0.0011)	(0.0034)
Observations	1232595	207023	337515	601793	86264
First child					
Age 1-5	-0.035***	-0.026***	-0.045***	-0.030***	-0.044***
	(0.00097)	(0.0024)	(0.0018)	(0.0013)	(0.0040)
Age 6-10	-0.049***	-0.031***	-0.053***	-0.038***	-0.044***
	(0.0014)	(0.0035)	(0.0027)	(0.0020)	(0.0060)
Age 11-13	-0.057***	-0.034***	-0.051***	-0.044***	-0.036***
	(0.0020)	(0.0046)	(0.0038)	(0.0029)	(0.0094)
Part time	0.017***	0.027***	0.025***	0.017***	-0.00033
	(0.00046)	(0.0012)	(0.00095)	(0.00060)	(0.0019)
Observations	1232595	207023	337515	601793	86264
First child					
Age 1-5	-0.030***	-0.019***	-0.038***	-0.025***	-0.044***
	(0.00096)	(0.0024)	(0.0018)	(0.0013)	(0.0040)
Age 6-10	-0.044***	-0.024***	-0.046***	-0.033***	-0.044***
	(0.0014)	(0.0035)	(0.0027)	(0.0020)	(0.0060)
Age 11-13	-0.052***	-0.027***	-0.045***	-0.038***	-0.036***
	(0.0020)	(0.0046)	(0.0038)	(0.0029)	(0.0094)
Public sector	0.0044***	-0.0067***	-0.0054***	0.021***	-0.0052*
	(0.00065)	(0.0015)	(0.0011)	(0.0010)	(0.0027)
Observations	1232595	207023	337515	601793	86264

Note: Each column in each panel provides FE estimates from a regression based on Equation 1 for the sample ofi ndividuals who had their first child in 1994 or later. Year dummies, a polynomial in age, individual fixed effects and extensive controls for further childbearing are included in all specifications. Each off our additional controls is included in the four different panels. Robust standard errors are in parentheses. * p < 0.10,** p < 0.05, *** p < 0.01.

Table 15: The effect of children on men's hourly wage. Including controls for experience, parental leave, part time and sector of employment in separate regressions.

	All	Secondary education		Higher education	
		Lower	Upper	Lower deg.	Higher deg.
	(1)	(2)	(3)	(4)	(5)
First child					
Age 1-5	-0.0060***	-0.00040	-0.0076***	-0.0057***	-0.0023
	(0.0012)	(0.0023)	(0.0019)	(0.0021)	(0.0041)
Age 6-10	-0.016***	0.00070	-0.012***	-0.012***	0.0027
	(0.0017)	(0.0034)	(0.0028)	(0.0031)	(0.0059)
Age 11-13	-0.024***	-0.00020	-0.016***	-0.016***	0.014
	(0.0025)	(0.0047)	(0.0041)	(0.0046)	(0.0087)
Experience	-0.0080***	-0.0014	-0.0019	0.0036	0.0023
	(0.0015)	(0.0028)	(0.0030)	(0.0025)	(0.0044)
Observations	1319323	321843	494896	393653	108931
First child					
Age 1-5	-0.0051***	0.00014	-0.0040**	-0.0018	0.0032
	(0.0012)	(0.0024)	(0.0020)	(0.0021)	(0.0042)
Age 6-10	-0.015***	0.0012	-0.0085***	-0.0086***	0.0080
	(0.0018)	(0.0034)	(0.0029)	(0.0031)	(0.0060)
Age 11-13	-0.023***	0.00027	-0.013***	-0.012***	0.019**
	(0.0025)	(0.0047)	(0.0041)	(0.0046)	(0.0088)
Parental leave	-0.013***	-0.0082	-0.047***	-0.039***	-0.046***
	(0.0028)	(0.0068)	(0.0054)	(0.0044)	(0.0074)
Observations	1319323	321843	494896	393653	108931
First child					
Age 1-5	-0.0062***	-0.00025	-0.0076***	-0.0054***	-0.0018
	(0.0012)	(0.0023)	(0.0019)	(0.0021)	(0.0041)
Age 6-10	-0.016***	0.00076	-0.012***	-0.012***	0.0028
	(0.0017)	(0.0034)	(0.0028)	(0.0031)	(0.0059)
Age 11-13	-0.024***	-0.00027	-0.016***	-0.015***	0.014
	(0.0025)	(0.0047)	(0.0041)	(0.0046)	(0.0087)
Part time	-0.00094	0.024***	0.025***	-0.019***	-0.047***
	(0.0014)	(0.0028)	(0.0030)	(0.0020)	(0.0046)
Observations	1319323	321843	494896	393653	108931
First child					
Age 1-5	-0.0062***	-0.00038	-0.0076***	-0.0057***	-0.0022
	(0.0012)	(0.0023)	(0.0019)	(0.0021)	(0.0041)
Age 6-10	-0.016***	0.00087	-0.012***	-0.012***	0.0027
	(0.0017)	(0.0034)	(0.0028)	(0.0031)	(0.0059)
Age 11-13	-0.024***	0.000036	-0.016***	-0.016***	0.014
	(0.0025)	(0.0047)	(0.0041)	(0.0046)	(0.0087)
Public sector	-0.0064***	-0.010***	-0.0027**	0.013***	-0.015***
	(0.00071)	(0.0017)	(0.0012)	(0.0012)	(0.0023)
Observations	1319323	321843	494896	393653	108931

Note: Each column in each panel provides FE estimates from a regression based on Equation 1 for the sample ofi ndividuals who had their first child in 1994 or later. Year dummies, a polynomial in age, individual fixed effects and extensive controls for further childbearing are included in all specifications. Each off our additional controls is included in the four different panels. Robust standard errors are in parentheses. * $p < 0.10$,** $p < 0.05$, *** $p < 0.01$.

Chapter 4

Random kids - causal inferences from using miscarriage as a natural experiment

Simen Markussen[1] and Marte Strøm[2]

Abstract The event of miscarriage has increasingly been used in the literature as random variation in the timing of children. We argue that miscarriage influences several measures of family structure; in addition to the timing of birth, it influences both the presence, the number, and age-distribution of children in the household. The causal effect of miscarriage on economic outcomes is therefore a combination of all these family measures, and using miscarriage as an instrument for only one is misleading. We estimate the reduced form impact of miscarriage on three measures of family structure and four measures of labor market outcomes. We find persistent differences in family outcomes five years after planned birth-year. The effect of miscarriage on labor market outcomes are largest in the first years, while the effect almost disappears 3-5 years after.

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4.1 Introduction

Miscarriage randomly prevents the birth of a child. It thus provides unique variation in whether a woman has a child at the planned point in time which we exploit to estimate the causal impact of an exogenous distribution of children on labor market outcomes. An emerging literature has used miscarriage as an instrumental variable for the timing of birth; Hotz et al. (1997, 2005) to study the effect of teenage childbearing, Miller (2011) to study the effect of having children later in the career for life-time earnings and Buckles and Munnich (2011) to study the effect of spacing of siblings for child school performance (identified by miscarriages before the second child). We argue that although miscarriage provides random variation in a field of study with few potential instruments (family and labor economics)[3], the causal inferences that may be drawn for labor market outcomes goes through the effect that miscarriage has on several family outcomes at the same time; whether an individual has children at all, timing of birth, number of children, spacing of siblings and age of youngest child[4].

The main contribution of this paper is to estimate the effect of miscarriage on different family outcomes and also on different labor market outcomes. This is similar to the approach in Rosenzweig and Wolpin (1980) who discuss the effect of having twins in the first birth and the effect it has on both subsequent fertility and female labor force participation. They find that having twins at the first birth substantially alters the life-cycle pattern of fertility but has only negligible impact on completed family size. The later impact on labor force participation, they therefore interpret to be the impact of exogenously distributing children over the life-cycle. In our case, miscarriage both leads to a postponement of children, fewer children, narrower spacing of siblings and for some no children at all, and the difference in family measures are substantial in the short run. We therefore use miscarriage as a proxy for exogenously distributing children for women who plan to have children at the same time.

We use Norwegian administrative data on all births in the period July 2001 to December 2003. The labor market outcomes that we look at are; earnings, labor market participation, weekly hours and hourly wages. We observe the individuals in a quite short interval - five years before birth and five years after birth. Because of the detailed data on short-run outcomes, we are able to track the relative importance of the different family measures for labor market outcomes. We compare the relative effect of miscarriage on labor market outcomes in the five years following planned birth to the relative effect of

[3]The most famous alternatives are maybe using twins as exogenous distribution of children (see for instance Bronars and Grogger (1994); Rosenzweig and Wolpin (1980)) and also the gender composition of the first two children to instrument the probability of having a third child (Angrist and Evans, 1998). Neither of these can be used to study the effect of the first child, which is potentially the most important

[4]Using miscarriage as an instrument for a single family outcome means implicitly to assume that the remaining fertility outcomes has no effect on the object of study. If this is violated the instrumental variable strategy will collapse the effects of all different fertility outcomes into an overstated effect from the instrumented variable.

miscarriage on the different fertility measures. We estimate separately the impact of a miscarriage before the first child and miscarriage before the second child to see whether the marginal impact of having a second child is different from the impact of having the first.

The impact of miscarriage on labor market outcomes are large in the first few years after planned birth. Those who give birth have lower earnings and work fewer weekly hours. After five years, the only significant difference in labor market outcomes is in weekly hours, where those who gave birth in the planned birth-year still work fewer hours. The small and insignificant differences in labor market outcomes 5 years after planned birth are remarkable since the two groups differ considerably along the measures of family structure. The fertility measure that changes most over the 5 first years is the probability of having any children. Among those who miscarry, 80% have children 5 years after. The differences in earnings in the last years indicate that whether the individual has children at all, which is closely correlated to having small children, has largest effect on earnings. The marginal effect of the second child is almost equally large, which indicates that having small children is what matters most.

We also test the randomness of miscarriage with respect to labor market outcomes before pregnancy. We find that there are small significant differences between those who miscarry and those who give birth. After controlling for observable differences in age, education and continent of origin, the group that miscarries are employed with a smaller probability and have somewhat lower wage-rates two years before planned birth. This indicates that the group that miscarries is also a negatively selected group, but the differences are not large, and the assumption that miscarriage is exogenous to labor market outcomes can be a good approximation.

4.2 Children and women's labor supply

Since Mincer (1962), labor supply of women has been analyzed in connection to their alternative use of time; in home-production (for instance taking care of children) in addition to leisure. This is also the basis of models of household production from Becker (1965, 1981) to Chiappori (1988). More home-production increases the value of home-time relative to time spent in market production and leads to a reallocation of hours from market production to home-production. The effect of having children on market labor supply will be negative. The more children an individual has or the younger they are, the more home-production, and the larger should the effect on market hours be.

The empirical evidence of a negative relationship between children (the presence, the number, and the age-distribution) and female labor supply is large (for a review, see Browning (1992)). With the lack of exogenous variation in either family or labor market outcomes, however, it is difficult to draw causal inferences. There are theoretical reasons for seeing family and labor decisions as joint decisions and therefore to doubt that the presence of children is exogenously given. With the reasoning of Becker (1965), an increase in real wages increases the value of time and therefore the cost of home production such as child-rearing. In addition, having children can be more expensive for high-income earners because they demand "higher quality" children (higher income families invest more in each child) (Becker and Lewis, 1973). Both considerations would give fewer children for higher-income families. As pointed out in Browning (1992), the two directions of causality is also reflected in different research traditions; labor economists treat children as an independent variable influencing labor market outcomes, while demographers treat labor market outcomes as the independent variable influencing fertility choices. The direction of the causal effect is therefore not obvious.

If miscarriage is truly random, it provides unique variation in the probability of having children at the planned point in time. The potential impact of economic factors on who becomes parents and at what time is equal for the group that miscarries and the group that gives birth. The only difference between the groups is that some have children while some do not in the planned birth-year. This will ensure the causal interpretation of the effect going from having children to labor market outcomes. New fertility decisions are taken every period, however, and the two groups will differ on several family outcomes in the years following.

Those who give birth will be younger when they have a child, they will have a higher probability of having children and have more children in the first years - and potentially permanently. The spacing between siblings is larger, and their youngest child is older. The fact that they are younger when they give birth will not have an obvious impact on hours worked. Both the higher probability of having children, the number of children and a larger spacing between children have an expected negative effect on labor supply. The fact that the youngest child is older has an expected positive effect. The random change we use in family structure should therefore affect those who have children in the planned birth-year negatively, maybe with a positive adjustment over time as the child(ren) grow older.

There is also a large literature on the negative wage-effect of children (Waldfogel, 1997; Lundberg and Rose, 2000; Budig and England, 2001; Datta Gupta and Smith, 2002; Anderson et al., 2002; Wilde et al., 2010; Cools and Strøm, 2012). Periods out of the labor market and periods of reduced working hours will adversely affect mothers' accumulation of human capital (Mincer and Polachek, 1974). We therefore expect to find negative wage-effects in the years following negative labor supply effects.

Table 1: Observations of birth and miscarriages in our data. All Norwegian women in the period 2001-2006

	First birth		Second birth	
	Birth	Miscarriage	Birth	Miscarriage
Observations in data	58,853	2,614	30,639	1,335
Age between 23 and 45 years	49,207	2,189	30,639	1,335
Employed in planned birth year	24,958	1,887	14,611	1,155
Miscarriage with specialist diagnosis		961		565
Observations in sample	24,958	961	14,611	565
Mean length of sickness absence with miscarriage		8.1 days		10.0 days
Share with children later		83.4%		77.4%
Mean length of postponement		15.0 months		13.4 months

4.3 The randomness of miscarriage

We first evaluate whether or not miscarriages can be considered exogenous to labor markets outcomes before planned birth-year.

The measure of miscarriage we use are taken from register data provided by Statistics Norway and the Norwegian Social Security Administration (NAV) on all sickness spells certified by a physician between May 2001 and December 2003. Table 1 shows a summary of the observations that we have. Our definition of miscarriage is based on the diagnosis ICD-10, code O03. This diagnosis is used by specialists at hospitals, which means that the women who miscarry in our sample has been to the hospital because of the miscarriage. Most probably, these miscarriages happen before 22 weeks after conception (when the diagnosis changes to still birth). The share of women registered with the specialist-coded miscarriage is quite small (4.2% both before the first and the second child - see table 1) compared to the high occurrence referred in the medical literature (around 10% of clinically recognized pregnancies do not result in the delivery of a baby (30% if you count early pregnancy loss) (Wilcox et al., 1988; Wang et al., 2003)). The main reason for this is that 3/4 of miscarriages happen the first trimester (before 12 weeks) and seldom lead to complications that need hospital-treatment. Our observations of miscarriage is therefore probably later than the first trimester. Since we do not know the date of conception, we set the length of pregnancy to 17 weeks (the middle between 12 and 22 weeks) at the time of miscarriage and calculate the expected month of birth based on this.

We have more observations on miscarriage (about as many as the ones we use - see table 1), but these are based on diagnosis from the physician. We exclude them from the sample because the selection into physician-prescribed sick-leave is not random. The pattern in fertility and labor market attachment for this group follows the same path as the group we include, however, which is an indication that the later miscarriages are not so different from earlier miscarriages on later outcomes.

According to the medical literature, miscarriage is in most cases caused by an anomality in the fetus (e.g chromosomal aberrations) or the mother has a physical defect (uterine anatomic defect) (Kline et al., 1989; García-Enguídanos et al., 2002). The impact of behavioral factors on miscarriage risk are small; extreme behavior like heavy alcohol drinking or drug use can lead to miscarriage, but this is very rare. Miscarriage risk increases dramatically with age, but age we observe and can control for.

In table 2 and 3 we test whether the group that miscarry is significantly different from the group that gives birth 2 and 3 years before planned birth-year. The first panel measures the difference in observable variables between the groups. We see there are significant differences in age, educational level and continent of origin. These variables we control for in later reduced form estimations, and do therefore not affect the validity of the estimates.

In the bottom panel of table 2 and 3, we test whether our measures of labor market outcomes in addition to sickness absence behavior are different in the groups 2 and 3 years before birth/miscarriage. If those who give birth have lower wages than those who miscarry even before the year of planned birth, there are probably some unobservable factors affecting both the probability of miscarriage and the outcome variables. The estimate of the effect of miscarriage will then be biased and will therefore also bias the results when using miscarriage as an instrument. We see that comparing the raw data in column 1 and 2, there are significant differences. These differences are only partly due to the observed differences in age, education and continent of origin as we show in the last three columns.

In order to remove observable differences between the birth- and miscarriage groups we first run a regression controlling for age (21 dummy variables), planned birth-month (30 dummy variables), continent of origin (5 dummy variables) and education level (9 dummy variables). These are the same observable variables that we control for in later estimations and graphical presentations. The error term from this regression is added to the sample mean before we divide in birth- and miscarriage groups and compare the group averages (column 4 and 5). The difference is shown in column 6. We see that after controlling for observables, the difference is smaller for some outcomes and the difference is insignificant for wage before the second child. There are however signs of a negative selection of those who miscarry: they are less employed, have lower wages and receive more social security before the first child.

The evidence in table 2 and 3 only partly supports the hypothesis that experiencing a miscarriage is truly random in the group that plans to have children, at least exogenous to labor market outcomes. This points to a general weakness of using miscarriage as an instrument.

There are potential problems that the group that miscarries will differ on other dimensions than in family structure. One is that miscarriage in itself will have career consequences apart from the fact that the individual doesn't have a child. If the woman e.g experiences a trauma following the miscarriage, this can have negative career consequences. The medical literature compares the period after a miscarriage to a period of grief where feelings of depression/anxiety is most pronounced the first six months after the experience and then wavers off, and is back to "normal" after a year (Broen et al., 2004; Lok et al., 2010) or six months found by others (Brier, 2008). We do not observe a jump in sickness absence following miscarriage (figure 2 and 3) which indicates that the negative consequences are not too large.

Another potential problem with miscarriage is that it is based on sick-leave. Even though the miscarriage is exogenous, it is not exogenous who is on sick-leave. We do, however only include sick-leave given after a hospital-treatment. Hospital treatment in connection to miscarriage is not decided by the woman herself but prescribed by a physician. The test we presented in table 2 and 3 show that those who experience a miscarriage have a very similar sickness history as those who do not experience a miscarriage, so it seems unlikely they are severely negatively selected on health.

4.4 Data

We use administrative data from official registries provided by Statistics Norway for our measures of demographical variables like age, education, region, the linking of children to their parents and work status information like being on welfare and participating in the labor force. The data on work status and welfare recipience gives us precise and detailed information on labor market dynamics of the individuals and makes it possible to track their status month by month. We use data from 1996-2008 to measure the effect five years before and after birth. The birth/miscarriages are observed from July 2001 to December 2003.

The frequency and the quality of our data provides us with unique opportunities to both test the exogeneity of miscarriage and also compare economic activity of the two groups before and after planned birth. The information on miscarriage is also from official registries on sickness spells. The good quality of the data is important in the question of miscarriage because survey data are often based on retrospective recollection. Retrospective reporting may suffer from measurement error and selection of who reports an experience of miscarriage in addition to self-justification bias (Bound, 1991).

Data on working hours and hourly wages are obtained from Statistics Norway's *Wage statistics* ("Lønnsstatistikken"), which is based on employer reports for a sub-sample of Norwegian enterprizes. Every year all public enterprizes are included, whereas a 50%

Table 2: Descriptive statistics of the group that gives birth and the group that miscarries, first child. T-test of whether there are significant differences between the groups in outcome-variables before planned birth-year. Both unconditional and conditional on observable differences.

	Raw data			Controlled for observables		
	Birth	Miscarriage	Difference	Birth	Miscarriage	Difference
Control variables						
Age	29.6	30.2	-0.63***			
Education	5.23	5.16	0.07			
Europe	9.1	9.0	0.1			
Asia	2.6	4.1	-1.5***			
Africa	0.7	1.1	-0.5*			
America	0.5	0.1	0.4*			
Outcome variables						
Employed						
t-2	95.1	93.5	1.6**	95.1	93.5	1.6**
t-3	90.6	89.6	1.0	90.6	89.2	1.4
Work hours						
t-2	32.6	33.0	-0.39	32.6	32.9	-0.29
t-3	31.8	32.5	-0.63	31.8	32.2	-0.34
Wage rate						
t-2	184.4	183.7	0.67	184.5	180.9	3.59*
t-3	182.0	184.4	-2.38	182.1	180.7	1.43
Sickness leave						
t-2	2.6	2.3	0.3	2.6	2.2	0.4
t-3	1.9	1.9	0.04	1.9	1.7	0.2
Social Security						
t-2	2.1	2.2	-0.1	2.1	2.1	-0.01
t-3	2.4	3.4	-1.0**	2.4	3.3	-0.8**

In order to remove observable differences between the birth- and miscarriage groups we first run a regression controlling for age (21 dummy variables), planned birth-month (30 dummy variables), continent of origin (5 dummy variables) and education level (9 dummy variables). The error term from this regression is added to the sample mean before we divide in birth- and miscarriage groups and compare the group averages. The difference is shown in the last column.
p<0.10, ** p<0.05, *** p<0.01 are results from a t-test of whether the difference is significant.

Table 3: Descriptive statistics of the group that gives birth and the group that miscarries, second child. T-test of whether there are significant differences between the groups in outcome-variables before planned birth-year. Both unconditional and conditional on observable differences.

	Raw data			Controlled for observables		
	Birth	Miscarriage	Difference	Birth	Miscarriage	Difference
Control variables						
Age	31.8	33.0	-1.27***			
Education	4.88	4.84	0.04			
Europe	7.7	8.8	-1.13			
Asia	2.7	4.1	-1.4**			
Africa	0.6	0.5	0.1			
America	0.3	0.9	-0.5**			
Outcome variables						
Employed						
t-2	92.8	89.6	3.2***	92.8	88.7	4.1***
t-3	88.6	88.3	0.3	88.7	86.9	1.7
Work hours						
t-2	30.4	31.7	-1.34***	30.4	31.4	-1.02**
t-3	30.7	31.3	-0.62	30.7	31.0	-0.3
Wage rate						
t-2	179.3	186.9	-7.6**	179.5	181.3	1.8
t-3	176.7	184.5	-7.8**	176.9	179.1	-2.2
Sickness leave						
t-2	3.0	3.1	-0.2	3.0	3.0	-0.06
t-3	2.0	1.7	0.3	2.0	1.5	0.5
Social Security						
t-2	3.6	4.2	-0.6	3.6	4.1	-0.5
t-3	3.9	4.1	-0.2	3.9	4.1	-0.2

In order to remove observable differences between the birth- and miscarriage groups we first run a regression controlling for age (21 dummy variables), planned birth-month (30 dummy variables), continent of origin (5 dummy variables) and education level (9 dummy variables). The error term from this regression is added to the sample mean before we divide in birth- and miscarriage groups and compare the group averages. The difference is shown in the last column.

p<0.10, ** p<0.05, *** p<0.01 are results from a t-test of whether the difference is significant.

sample of medium size enterprizes and a 20% sample of small enterprizes is drawn every year for the private sector[5]. In total 70% of the Norwegian labor force is covered in the Wage statistics every year.

Our sample consists of women who had a child and/or experienced a miscarriage in our time-window. We only include observations on women who work (earn more than 1G[6]) because the data on miscarriage is only available for those who have a job - and therefore can get sickness leave.

In our analysis, we study the main activity on the labor market the first 5 years after planned birth and the five years before. The different states we study are employed, on parental leave, on pregnancy related sickness absence, on other type of sickness absence, on welfare benefit or inactive. We include all states if an individual is observed in several, but being on parental leave is excluded from our measure of employment (if the individual is on parental leave, we set employment to missing).

Employment is defined as earning more than 1G. Sickness absence is registered absence more than 16 days (the first 16 days are paid by the employer and is therefore not registered in the official data on sickness-payments)[7]. Sickness absence observed the last nine months before a birth, we define as pregnancy related sickness absence. Other welfare benefits are all registered benefits excluding benefits that are connected to having children. We define the individual as inactive if she is not observed in any other activity.

4.5 Empirical specification

We estimate a reduced form impact of miscarriage on measures of family structure and labor market outcomes. The effect of miscarriage on the different family measures, we interpret as the importance of the different channels through which miscarriage can have a labor market impact.

The setup is a linear regression of outcomes on a dummy for miscarriage, controlling for observable characteristics:

$$\mathrm{Y}_{t+x} = \beta_0 + \beta_1 \mathrm{Miscarriage}_t + \beta_2 \mathrm{X}_t + u_{t+x} \tag{1}$$

where Y_{t+x} are the outcomes that we study: 3 family measures: have children, number of children and the age of the youngest child, and 4 labor market outcomes: labor market participation, earnings, weekly hours and hourly pay.

[5]Employment in agriculture, hunting and forestry is left out. So are enterprizes with 3 or less employees.

[6]The basic amount G (*Grunnbeløpet i folketrygden*) is adjusted for wage inflation, usually once a year. From May 2011 the amount is 79 216 NOK, approximately 14 400 USD.

[7]Note that these data are different from the ones used to identify miscarriages. Whereas the data for sickness-payments cover only spells lasting more than 16 days the data used for identifying miscarriages cover all spells from the day a physician in visited. The reason why we use data on sickness payments is that these are the only data on sickness absences we have available for the years 2007 and 2008.

X_t are our observable characteristics, including age (21 dummy variables), planned birth-month (30 dummy variables), continent of origin (5 dummy variables) and education level (9 dummy variables). The observable characteristics are measured in the year the individuals have a child or should have had a child had they not miscarried.

β_1 is our parameter of interest; the effect of miscarriage on the outcome variables. It represents the difference in outcomes for those who give birth compared to those who miscarry. The estimated effects on family outcomes are representative of potential first step regressions in an instrumental variable set-up, e.g. the effect of miscarriage on the probability of having children:

$$\beta_{1children} = \left(\frac{cov(children, miscarriage}{var(miscarriage)} \right) \tag{2}$$

The estimated effects on labor market outcomes are representative of the reduced form estimation of the effect of miscarriage, e.g. the effect of miscarriage on wage:

$$\beta_{1wage} = \left(\frac{cov(wage, miscarriage}{var(miscarriage)} \right) \tag{3}$$

In an instrumental variables set-up, the effect of a family outcome on labor market outcomes is the reduced form coefficient β_{1wage} divided by the first step coefficient $\beta_{1children}$. As miscarriage influences several family measures at the same time, using it as an instrumental variable for only one family outcome is not valid. We may however interpret the reduced form effects as a weighted mean effect of miscarriage, and the effect on the different family measures as potential channels.

We present all results graphically as well to show the levels. In the figures, the predicted mean values for the treatment and control group are shown, corrected for differences due to the observable characteristics. The 95% confidence interval around the means are shown as well. In addition, we present graphically the effects on 6 economic activity measures: Labor market participation, sickness-absence, pregnancy related sickness absence, on parental leave, overall benefit dependency and inactive.

The effects on economic activity represents what is the activity of the treatment and comparison group. The impact on the labor market outcomes should be interpreted in light of what it means to economic activity to have a miscarriage. Miscarriage means in most cases a postponement of children which includes periods on parental leave and non-employment. The economic activity of the two groups will tell us when the comparison of labor market outcomes is relevant.

To see whether the marginal impact of the second child is the same as the first, we use miscarriages both before the first birth and the second birth.

Table 4: The effect of miscarriage on family outcomes

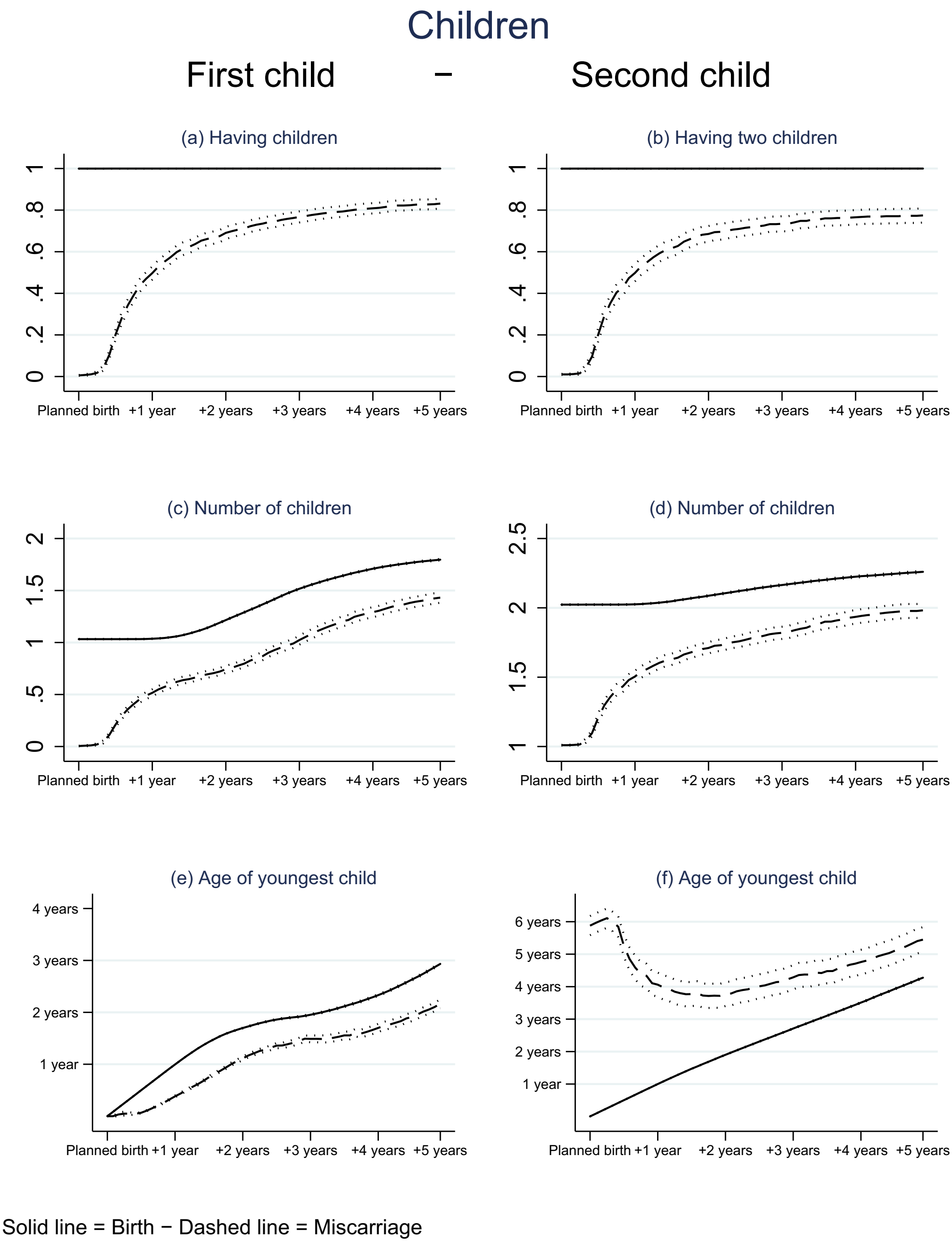
	t+1	t+2	t+3	t+4	t+5
Panel A: First child					
Having children	-0.53***	-0.33***	-0.24***	-0.19***	-0.17***
	(0.0032)	(0.0030)	(0.0027)	(0.0025)	(0.0024)
Number of children	-0.55***	-0.47***	-0.51***	-0.42***	-0.37***
	(0.0069)	(0.014)	(0.018)	(0.018)	(0.018)
Age youngest child (in months)	-7.16***	-7.26***	-5.31***	-7.58***	-9.26***
	(0.023)	(0.28)	(0.50)	(0.58)	(0.62)
N	25919	25919	25919	25919	25919
Panel B: Second child					
Having 2 children	-0.53***	-0.32***	-0.27***	-0.24***	-0.23***
	(0.0041)	(0.0039)	(0.0037)	(0.0035)	(0.0035)
Number of children	-0.55***	-0.38***	-0.35***	-0.29***	-0.28***
	(0.0079)	(0.012)	(0.016)	(0.019)	(0.020)
Age youngest child (in months)	38.7***	22.9***	19.2***	15.4***	14.2***
	(0.45)	(0.49)	(0.59)	(0.70)	(0.79)
N	15176	15176	15176	15176	15176

OLS regression of the different fertility outcomes on miscarriage, controlled for age (21 dummy variables), planned birth-month (30 dummy variables), continent of origin (5 dummy variables) and education level (9 dummy variables).
p<0.10, ** p<0.05, *** p<0.01

4.6 What miscarriage does to family structure - potential first stages

In this section, we show how the group that miscarries differs from the group that gives birth in three measures of family structure up to five years after planned birth year; having children, number of children and age of the youngest child (see Table 4). In addition, a miscarriage has an effect on the spacing of siblings for those we observe have more children in our time-window (First birth: 4.6 months closer between the first and second child, 3.8 months closer between the second and the third child. Second birth: 2.5 months closer between the second and the third child). The impact of miscarriage on all these measures shows that it has potentially a strong first-stage impact if it is to be used as an

Figure 1: Family outcomes for birth and miscarriage group



Mean of outcome variable, corrected for observable differences: age (21 dummy variables), planned birth-month (30 dummy variables), continent of origin (5 dummy variables) and education level (9 dummy variables)

instrumental variable. It does also however, show, that it has impact on several variables at the same time. It is not possible to identify the effect of one from the other without for instance putting more theoretical structure on the empirical analysis or including more instruments.

The overall picture of fertility after miscarriage is that a miscarriage is mainly a postponement of children. 76% of those who miscarry have a child within the three first years, and after five years, 83% have children. The differences we observe in labor market outcomes 5 years after planned birth, we therefore interpret to come mostly from the variation in number of children and age of the youngest child.

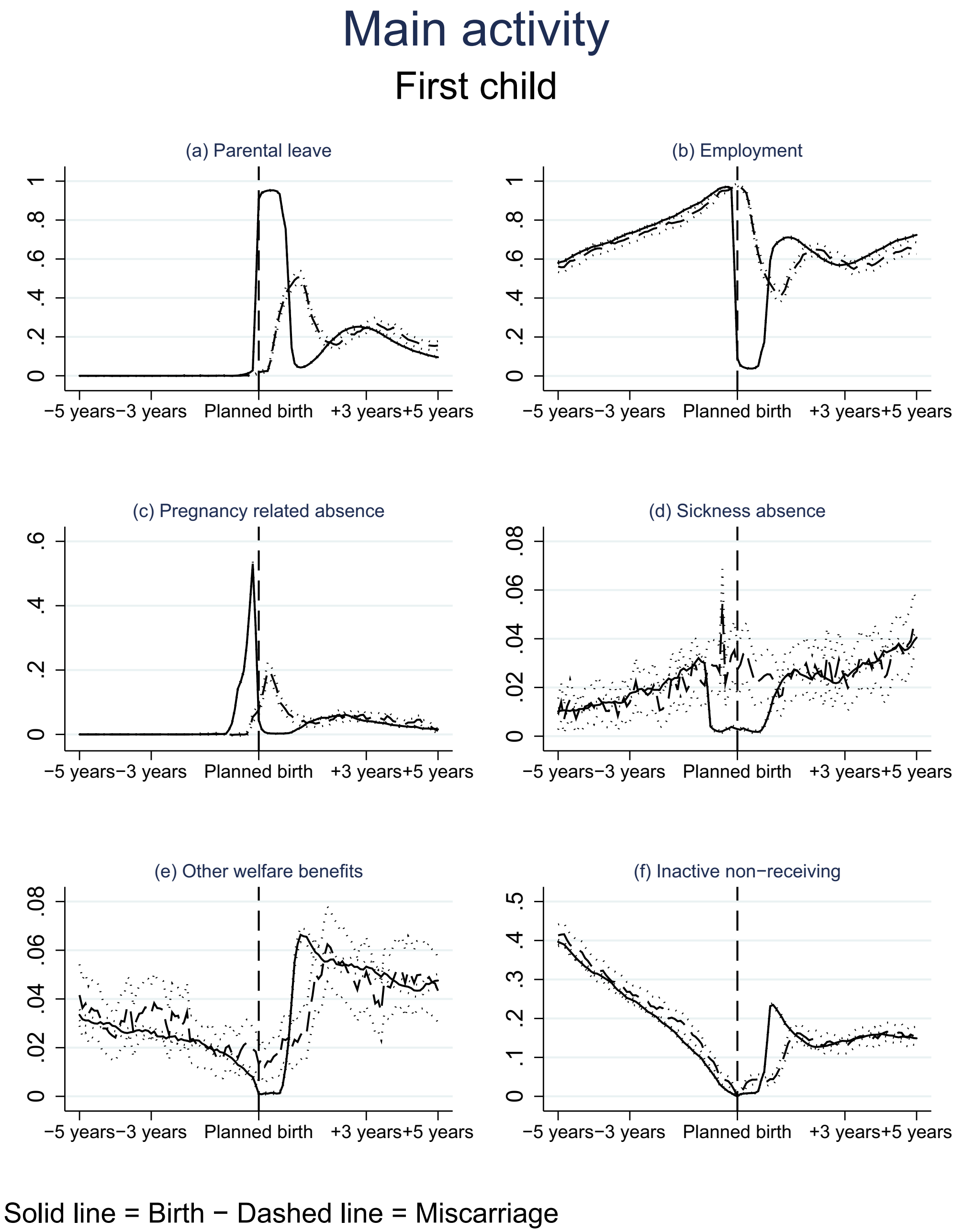
The data illustrates another important dimension of the decision to have children. It is unlikely that the decision to have children is only taken once - in the first period - and that when the child comes after that (in case of miscarriage) is random. Fertility decisions are taken every period, which also makes it hard to measure the causal impact of one periods outcome of a pregnancy several years after.

4.7 Main economic activity

Figure 2 displays the fraction on parental leave, the employment rate, the fraction on sickness-leave (during pregnancy and otherwise), the fraction who's main income stem from other social insurance programs and finally the fraction of people not belonging to any of these groups denoted "inactive". At least four important lessons can be drawn from this figure. First, the birth-group and the miscarriage group are as good as similar prior to the first pregnancy. Secondly, the groups are highly different the first two years after planned birth. These differences mainly stem from a postponement of birth. We see that the group that experiences a miscarriage postpones giving birth by approximately one year. At first sight one could imagine that a causal effect of having children could be estimated for the first year after a miscarriage since the difference in child outcomes in this period is truly random. However, as indicated by this figure, one would then compare the women just ended their parental leave to the miscarriage group where around half of the sample that miscarry are on parental leave in year t+2. The estimated effect of having children in year t+1 will be the estimated effect of "having children and coming back from one year parental leave" compared to "around 50% probability of being pregnant". The short-term causal impact of having children is therefore based on an "apples and pears-comparison" and is not providing much insight into the labor market consequences of having children.

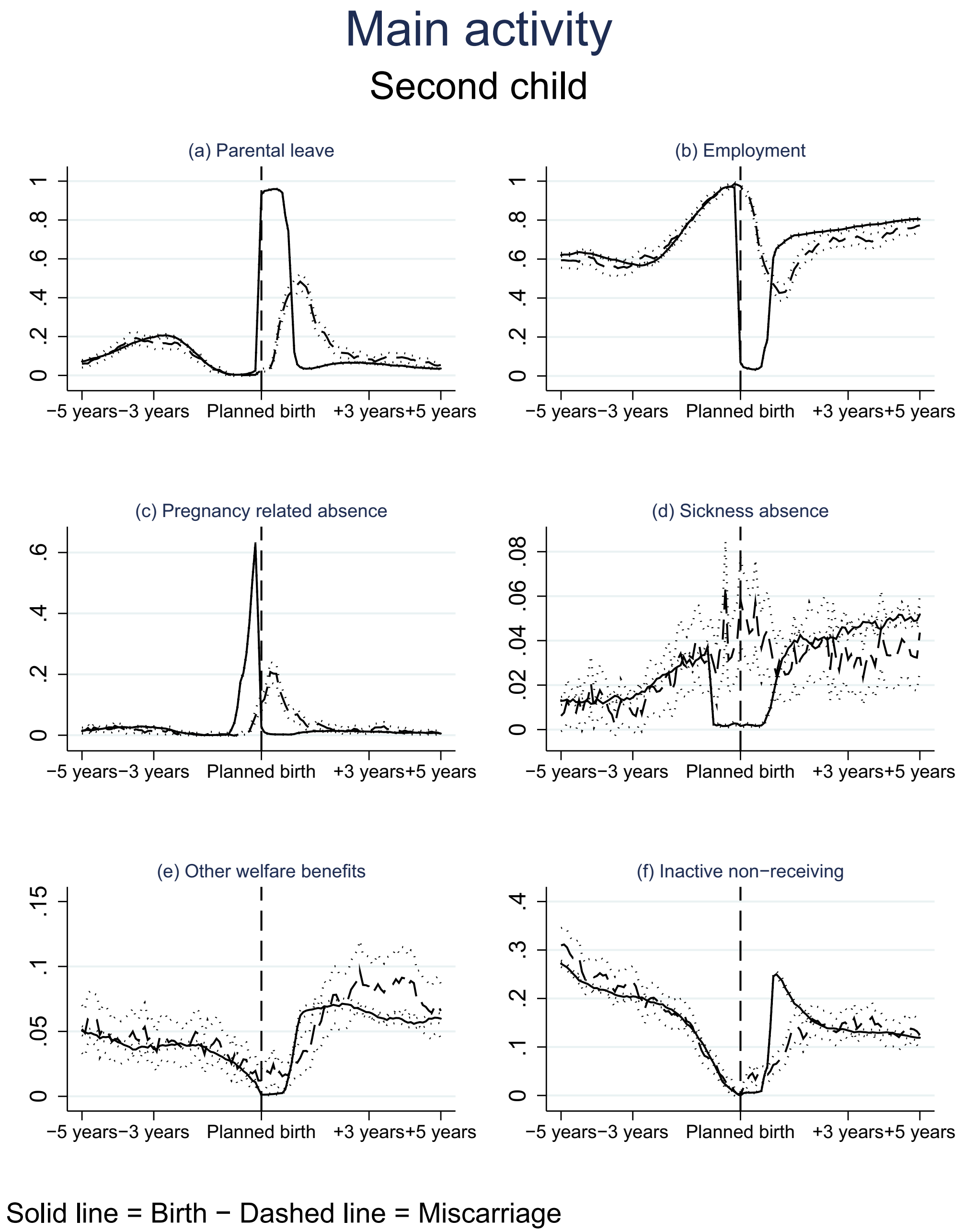
Thirdly, and perhaps the most interesting lesson from Figure 2 is that the long-term consequences of a miscarriage on labor market status are small, despite substantial differences in fertility outcomes. Five years after the first planned birth, the group that miscarries has more seldom, fewer, and younger children, but after finishing pregnancies

Figure 2: Main economic activity for birth and miscarriage group, first child



Mean of outcome variable, corrected for observable differences: age (21 dummy variables), planned birth-month (30 dummy variables), continent of origin (5 dummy variables) and education level (9 dummy variables)

Figure 3: Main economic activity for birth and miscarriage group, second child



Mean of outcome variable, corrected for observable differences: age (21 dummy variables), planned birth-month (30 dummy variables), continent of origin (5 dummy variables) and education level (9 dummy variables)

and then parental leave(s), the two groups are similar. Despite substantial short-term consequences of fertility, the long-term consequences for labor-market participation and social benefit uptake of having children are rather small.

Finally, an interesting pattern to note, is the pattern in sickness absence. It has been argued that the combination of career and family gives women a ”double burden” that can cause health problems - which in turn could explain the gender gap in sickness absence that is observed in Scandinavia as in many other countries. The empirical evidence of such a burden is mixed. Some studies support it (Åkerlind et al., 1996; Bratberg et al., 2002) while other studies find no association between having children and sickness absence (Voss et al. (2008), see also literature reviewed in Allebeck and Mastekaasa (2004)) or even a positive association (Mastekaasa, 2000; Bratberg et al., 2002).

The figures 2 and 3 show that although the two groups differ in number of children, there is no difference in sickness absence between the groups 3-5 years after the planned birth of the first child. There are however signs of the group having more children also having a higher level of sickness absence after the second child. This indicates that we cannot reject the hypothesis of a ”double burden” giving rise to health-problems when women have more than one child.

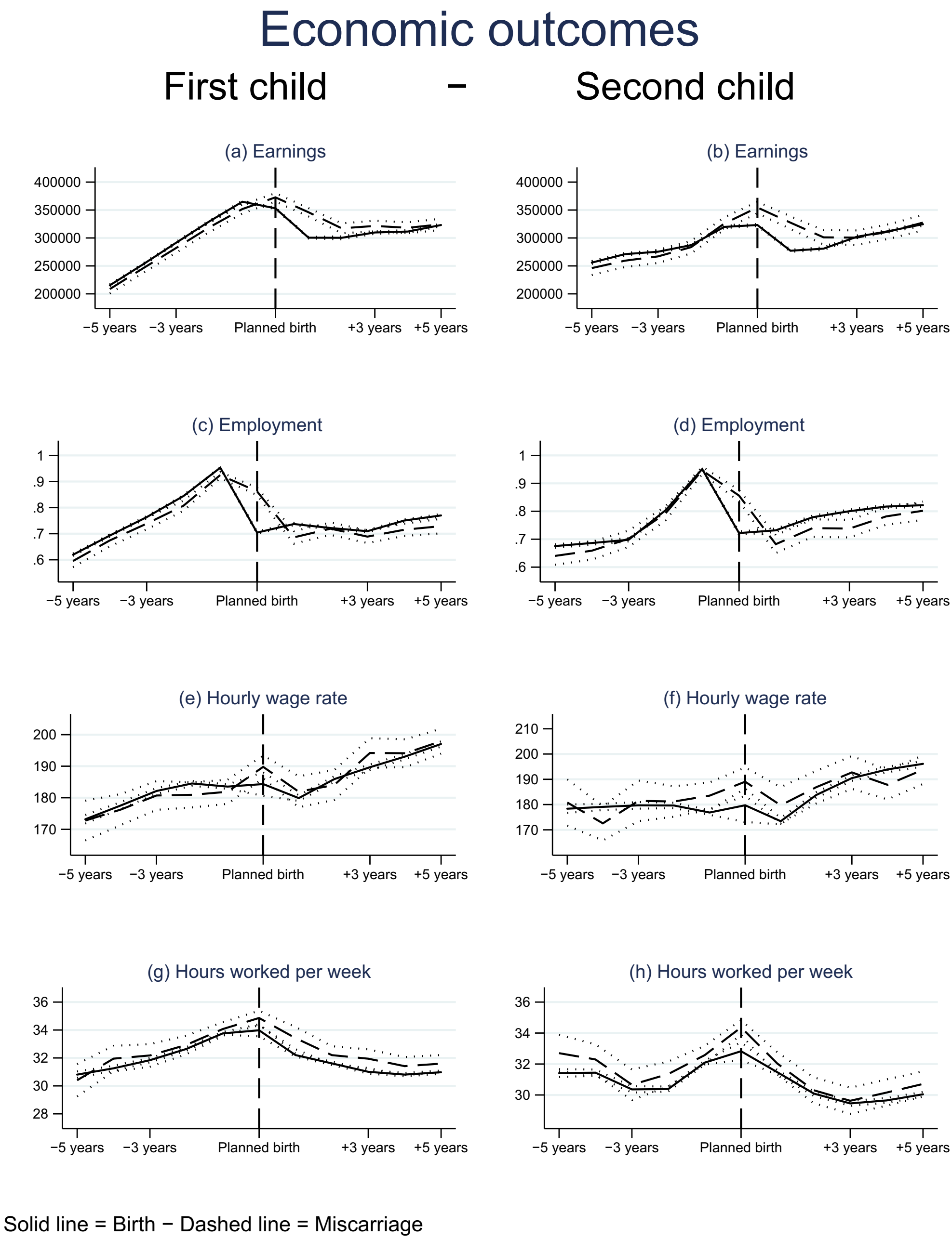
4.8 Labor market outcomes

As we now move on to study four economic outcomes in the labor market; employment, earnings, hours worked and hourly wages, the pattern we observe is similar to the one in the previous section. The two groups are similar prior to the birth/miscarriage. Then, as only one of the groups give birth at time t, substantial differences between the groups arises. Those who give birth have considerable lower earnings than those who miscarry, but this effect is short lived. The effect is similar after the first and the second child, and means that having small children has substantial, but temporary, effects on earnings.

When it comes to employment, those who miscarry have a lower probability of being employed all the five years after. This effect is largely due to the miscarriage group having a higher probability of being pregnant and being on parental leave as we saw in section 4.7.

For those who have returned to work, we see there is particularly one outcome, hours worked, for which we can see a permanent difference between the groups. Those who give birth at time t, and because of this has more (and more often) children than those who miscarry, work fewer hours five years later. The estimated difference for the first child is around 0.6 hours per week. There is the same difference in hours after the second child, but this is not significant. The similarity of the pattern after the first and the second child gives support to the hypothesis that it is the number of children that matters and not

Figure 4: Economic outcomes for birth and miscarriage group, second child



Mean of outcome variable, corrected for observable differences: age (21 dummy variables), planned birth-month (30 dummy variables), continent of origin (5 dummy variables) and education level (9 dummy variables)

Table 5: The effect of miscarriage on labor market outcomes

	t+1	t+2	t+3	t+4	t+5
Panel A: First child					
Earnings	45730.2***	17122.9**	11739.8*	6818.2	1255.8
	(3967.9)	(6717.2)	(6322.5)	(6496.0)	(6041.0)
N	25736	25527	25469	25708	25828
Employed	-0.052***	-0.0026	-0.021*	-0.036***	-0.041***
	(0.012)	(0.012)	(0.013)	(0.012)	(0.014)
N	25919	25919	25919	25919	25919
Weekly hours	1.23***	0.60*	0.95***	0.61*	0.62*
	(0.36)	(0.32)	(0.34)	(0.33)	(0.33)
N	13848	12509	12348	14307	15458
Ln hourly wage	2.04	-1.87	4.58**	1.03	0.90
	(2.88)	(2.37)	(2.32)	(2.33)	(2.32)
N	13806	12504	12347	14307	15458
Panel B: Second child					
Earnings	50878.9***	20362.3***	-1449.2	-896.4	2964.2
	(5320.2)	(6306.8)	(6501.7)	(6768.3)	(7173.9)
N	15128	14977	14975	15090	15133
Employed	-0.050***	-0.039**	-0.063***	-0.036**	-0.020
	(0.016)	(0.016)	(0.015)	(0.015)	(0.016)
N	15176	15176	15176	15176	15176
Weekly hours	0.50	0.25	0.18	0.52	0.65
	(0.45)	(0.43)	(0.44)	(0.43)	(0.41)
N	8171	8987	9320	9599	9843
Ln hourly wage	6.59*	3.32	2.25	-6.26**	-2.35
	(3.89)	(3.15)	(3.04)	(3.02)	(2.95)
N	8147	8982	9320	9599	9843

OLS regression of the different labor market outcomes on miscarriage, controlled for age (21 dummy variables), planned birth-month (30 dummy variables), continent of origin (5 dummy variables) and education level (9 dummy variables).
p<0.10, ** p<0.05, *** p<0.01

the age of the youngest child, because the effect of miscarriage on the age of the youngest child is exactly opposite after the first and the second child (see Table 4).

The results for wages are imprecise. Those who give birth in the planned birth-year have lower wages than those who miscarry three years after, and also the first year after the second child. It seems, however that they catch up, and have higher wages than those who miscarry four years after. Having (more) children has therefore only a temporary negative effect on wages. After the children have grown older, the wages catch up.

4.9 Conclusion

We have shown that miscarriage is close to a natural experiment, providing random variation in the probability of having children at the planned point in time. We argue, however that when using this random variation to measure impacts on labor market outcomes, miscarriage should be used as a proxy for several fertility measures at the same time. Miscarriage influences whether an individual has children at all, the timing of children, the number of children and the age-distribution of the siblings. The causal impact of miscarriage is therefore on a more broader definition of family structure - and the resulting impact on labor market outcomes a weighted average of all.

Instead of assuming that the miscarriage works through only one of the several possible "fertility channels" we study the reduced form differences in fertility outcomes as well as economic-/labor market outcomes between those who give birth and those who miscarry. There are mainly three lessons to learn from undertaking such comparisons: First, having a miscarriage has what seems like permanent consequences for fertility outcomes. 1 out of 5 having a miscarriage still has no children five years later. The number for those who miscarry at second birth are approximately the same. Furthermore, those who miscarry also has fewer children and for those that have more than one child, the spacing between their children is shorter than for others.

Secondly, the labor market consequences of having children are dramatic in the short run, when there is a much higher probability of having children in the group that gives birth as planned. This indicates that the presence of small children has direct effects on the time-allocation of women, and that children matter on the having-not having margin, not in numbers or distribution.

Thirdly, and perhaps also more surprisingly, despite permanent consequences for fertility it seems to be very few such long term consequences for labor-market outcomes. Whereas i.e. employment and sickness absence are dramatically affected during pregnancies and the first one or two years after birth, those who miscarry and those who give birth are almost identical 5 years later, regardless of whether we compare wages, earnings, employment, sickness absence or social insurance dependency.

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Chapter 5

How husbands and wives vote

Marte Strøm[1]

Abstract Political preferences depend on income. In a household, the income of the individual is part of the income of the whole household. I investigate empirically the importance of individual vs household income, and find that individual income is important if it is representative of the household income. If the wife is the maximum earner of the household or works fulltime, she votes more according to her own wage, similar to men. On average women earn less than their husband and vote according to their husbands income. Household income is therefore the best predictor of average voting behavior.

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Introduction

The rich and the poor have different interests, and this is the foundation for political economy models of voting behavior. Basic models, like the median-voter model (Meltzer and Richard, 1981), emphasise private income relative to the median as important for the preferred degree of redistribution or level of public services. The basic insight is that redistribution is less beneficial for high-income earners (with proportional taxation) who therefore demand lower levels of redistribution. These models abstract from the fact that most individuals are part of a household, and that this household will influence your economic position and/or your political views. Most major surveys on voting behavior (like the World Values Survey, the Eurobarometer, American National Election Studies and a number of other election studies) take the other extreme and only ask for household income.

The unitary model of household behavior (Becker, 1974, 1985) suggest that household income determines the vote. The usual income pooling result would apply, and it does not matter who of the couple has the highest income. The literature on household decision-making has moved beyond the Becker-theory of unitary decision, however. Bargaining theories (Manser and Brown, 1980; McElroy and Horney, 1981) and the theory of the collective model (Chiappori, 1988) model the household rather as two individuals making individual decisions, taking into account the effect of the decision on the individuals position in the household. The median voter model explicitly models the vote as demand for redistribution and/or public services. As the degree of redistribution and public services has a direct impact on the outside option of the spouses, bargaining or collective theories of household decision making should also apply to political voting behavior. The spouse with the lowest incomes could potentially have other political interests than the spouse with the highest income.

I use the National Child Development Study (NCDS) and the British Cohort Study (BCS) which are detailed survey data on two British cohorts born in 1958 and 1970. The data are unique for my purpose, as it is the only dataset to my knowledge that contains both individual income and spouse income in combination with individual voting behavior. In addition, it is panel-data, which enables me to investigate the role of income over the life-cycle. The empirical method I use is OLS regression to estimate the quantitative importance of own versus household income.

I show that predictions of voting behavior based on individual income and household income give very different results, especially for women who are not the main earners of the family. I find that individual income is only important for women if their income is fairly representative of the household - if they work full-time or earn a higher income than their husband. Otherwise, their husband's income has a much larger impact. Men always vote according to individual income. Even in the cases where he earns less than his wife, his wife's income has no significant impact on his voting behavior. On average, household income is therefore the best predictor of both men and women's voting behavior, but this is mainly the result of women's average economic position in the family.

Women vote more independently from their husband's economic interests if she herself is economically independent. As women on average earn less than men, a rise in female labour force participation can in the aggregate lead to a divergence of men and women's voting behavior. A modern[2] gender gap in voting (where women have become more leftist relative to men) is observed in all OECD countries over the last decades (Inglehart and Norris, 2000; Box-Steffensmeier et al., 2004a; Norrander and Wilcox, 2008). Edlund and Pande (2002) have found that rising divorce rates actually account for a large share of the increasing US gender gap in voting from 1964-1996, and find similar results for Europe (Edlund et al., 2005). A larger impact of individual income on voting behavior for women is a possible mechanism. I find some empirical support for this hypothesis using divorce next period as a proxy for perceived higher individual risk of divorce. Women who divorce next period are on average less conservative, and also vote relatively more according to own income.

The impact of income on voting behavior is sensitive to what age income is measured. I show that using a measure of permanent income, the average income when the individuals are 33 and 41, increases the impact of income for both men and women over the life-cycle. According to Solon (1999), the end of the 30's, beginning of the 40's is the age when actual income is closest to permanent income. The larger impact of income when using the measure of permanent income indicates therefore that permanent income is more important for voting behavior than transitory income. It may also be that this measure corrects some of the measurement problems of income that could potentially give a downward bias to the estimate.

Literature

There is to my knowledge no investigation of the importance of individual vs household income on political preferences in the literature. Most studies that study egotropic voting[3] use survey data that have information about household income, but interpret the results as if the income was representative of the individual. There are not many empirical studies of the importance of income on voting at all. Lind (2007, 2010) are exceptions, and find a positive correlation between income and conservative voting for Norwegian voters. Other studies have found support for egotropic voting on preferences for various welfare

[2]During the postwar era, women were more conservative than men in all western democracies. This changed from the beginning of the 1980's and women became increasingly more leftist relative to men (Inglehart and Norris, 2000)

[3]Egotropic voting refers to the importance of personal income for individual voting. Sociotropic voting refers to the importance of the macro-economy for individual voting

measures like redistribution (Husted, 1989; Ribar and Wilhelm, 1999), taxes (Lewis, 1979; Furnham, 1984) and provision of public goods (Preston and Ridge, 1995).

There is some evidence of women voting less egotropic than men. Norrander and Wilcox (2008) find that income was traditionally a stronger predictor of men's rather than women's ideology in Europe, but that the difference has declined over the period 1972-2000. Welch and Hibbing (1992) investigate the gender differences in economic voting, and find that women cast less egotropic, and more sociotropic votes than men. These studies are based on measures of household income and the estimates might therefore be biased downward for women because household income is a better predictor of the husband's personal income than the wife's personal income (more measurement error in the estimate for women e.g).

Several studies indicate that women are more influenced by their husbands social class when casting their vote than vice versa (and social class and income is usually closely related). For instance Erikson and Goldthorpe (1992) attempt to find which social class women identify with. Among other things, they investigate whether political attitudes are determined by their own social class or their husbands social class (defined by profession), and find a large impact of the husband's social class. Dirk De Graaf and Heath (1992) investigate the heterogeneity of the effect of own and spousal social class on political voting behavior, and find that women in the service class or blue-collar occupations take more account of own class than their husband's class, while women in the petty bourgeoise and routine non-manual class take relatively little account of their own class positions. Among men, they find no big differences according to class in how they weight their wife's class.

There is also a strand of literature on the increasing gender gap in voting (women became more left-wing relative to men) from the 1970's to 2000 on the aggregate level. Structural-economic explanations have been increasing female labour market participation and preferences for social spending since women who enter the labour market have lower average wages and have a higher demand for kindergardens/elderly care (Iversen and Rosenbluth, 2006; Manza and Brooks, 1998). Lott and Kenny (1999) studied the introduction of female voting rights in the US in the beginning of the 1900 and found a significant shift towards more redistribution. Other structural-economic explanations have been the growing number of economically vulnerable single women/lone mothers (Box-Steffensmeier et al., 2004b) and the higher risk of worse economic outcomes accompanying the higher divorce rates (Edlund and Pande, 2002). These studies look at the aggregate difference in voting behavior of men and women, explaining the trend with aggregate measures. They all point to a larger role of the individual compared to the household for political preferences.

A theoretical framework

To illustrate the importance of individual versus household income for economic voting behavior, I introduce household income and a probability of divorce in a textbook median-voter model of the demand for redistribution (Persson and Tabellini, 2002). I contrast the predictions of looking at household income as pooled income and of looking at it as two separate incomes of the spouses (like in the bargaining models or collective models of the family).

As the vote is cast individually, I model the vote as an individual decision, but take into account the possibility that consumption is dependent on total income of the couple. Individuals are part of a household, and income is therefore in part the income of the individual Y_i and in part the income of the spouse of individual i; Y_s. There is also a probability, $p > 0$, of getting divorced and only getting your own income. For simplicity, we assume risk neutrality in consumption.

The individuals' utility function is simple:

$$U_i = E(c_i) + \ln \text{g} \tag{1}$$

$$c_i = \begin{cases} (1-t)(\alpha Y_i + (1-\alpha)Y_s) & \text{if married/cohabiting} \\ (1-t)Y_i & \text{if divorced} \end{cases} \tag{2}$$

This gives the expected consumption: $E(c_i) = (1-p)(1-t)(\alpha Y_i + (1-\alpha)Y_s) + p(1-t)Y_i$. In addition to getting utility from consumption, c_i, individuals get utility from a public good, ln g, a concave and increasing function[4]. The parameter $0 \leq \alpha \leq 1$ represents the degree of income pooling in the household. If $\alpha = 1$, the individual only consumes according to own income, and the preferred level of consumption and public goods would be the same whether the individual was married or not. Every individual in the economy gets the same amount of public goods and the provision of public goods is financed by a proportional income tax where t is the marginal tax rate. The expected value of Y_i is $E(Y_i) = Y$, and the government budget constraint is $tY = g$.

Putting the individual's and the government's budget constraint into the utility function gives us the following:

$$U_i = (1-p)(Y-g)\left(\frac{\alpha Y_i + (1-\alpha)Y_s}{Y}\right) + p(Y-g)\left(\frac{Y_i}{Y}\right) + \ln \text{g} \tag{3}$$

Maximizing this equation with respect to g gives the following policy preferences of the individual that determines uniquely the individuals' demand for public goods as a function

[4]ln g could also be modelled as different whether the individual is married or not. E.g the utility the individual gets from a certain level of public services like kindergartens or elderly care can be higher if the individual is not married. This would give even larger weights on individual income for voting behavior.

of relative income:

$$g_i = (1-p)\left(\frac{Y}{\alpha Y_i + (1-\alpha)Y_s}\right) + p\left(\frac{Y}{Y_i}\right) \tag{4}$$

Since policy preferences are monotonic in the relative income of the individual, the demand for g is an inverse function of the price, which is higher the higher is the relative income of the individual to the median income (because taxes are proportional).

The role of partner's income in the demand for redistribution depends on two things in this setup. The first is the share α which represents to what extent the household pools income and consumption. If the household shares everything equally, the share will be $\frac{1}{2}$ and the marginal impact of each partner's income on the demand for redistribution will be the same. If the share is bigger than $\frac{1}{2}$ (the individual consumes more according to own income relative to partner's income), individual income will be relatively more important for the demand for redistribution. As women are not the prime-earners of the family on average, household income will be more representative of the individual income of the husband. If only household income is measured in the data, the lower degree of egotropic voting of women will merely be an artifact of the fact that household income is not close to their individual income.

The second role of partner's income on the demand for redistribution is the probability of divorce[5]. If the probability of divorce is larger than zero, the individual income is more important for the vote than the partner's income, reflecting Edlund and Pande (2002); Edlund et al. (2005)'s result that the gender gap in voting behavior has been increasing with the rising divorce rates.

In my set-up, it is the pure economic conditions that determine the vote. Individuals follow their own political preferences determined by income - and income may be earned by the individual herself/himself or by the spouse. In a more general household model of voting behavior, the preferences of an individual may depend on the preferences of the spouse. For instance through discussions in the household, information is shared and political views can be adjusted. If the political views of both spouses nevertheless depend on their individual income, the conclusions from this kind of model would be the same as inserting both incomes directly into the individual utility function. The interpretation will be slightly different though. The impact of spouse income will work through the impact that spouse income has on spouse political preferences - and the impact that spouse political preferences has on individual political preferences.

[5]The probability of divorce could also enter the sharing weights of the household, like in a collective model of household behavior (Chiappori, 1988). This would potentially give divorce a multiplier effect making individual income have an even larger impact.

Empirical specification

I estimate a linear regression model of voting behavior, where policy preferences for redistribution are represented by the vote cast in the last general election. The dependent variable is a dummy for whether the individual voted Conservative or not, and the coefficients can therefore be interpreted as the impact of the explanatory variable on the probability of voting Conservative.

The reason for using the vote as representing the individuals preferences for redistribution is an argument of revealed preferences. An answer to a survey question is not binding and involves no cost to the individual, whereas the vote does[6].

To estimate the role of individual vs household income, I estimate the model using different measures of income; in specification (1) I use household income, in specification (2) I enter both individual and spouse income separately to see whether these have different impact. In specification (3) I enter the maximum income and the minimum income of the household separately to see whether the differential impact of individual and spouse income is explained by which income is highest.

I use OLS-regression methods which gives the estimated impacts on the probability for an average individual. I cluster the standard errors at the individual level.

The policy preferences of the individual is represented by this equation:

$$\text{Conservative}_{it} = \alpha + \beta_1 \ln \text{Income}_{it} + \beta_2 \text{Wealth}_{it} + \beta_3 \text{Religion}_{it} + \beta_4 \text{Region}_{it} + \beta_5 \text{Year}_t + u_{it} \tag{5}$$

I do not control for educational level of the individual because this is in itself a proxy for permanent income. I have included education level as a robustness check however, and the results are largely unchanged.

Wealth_{it} is a dummy equal to one if the household owns its own house. This is a proxy for wealth which I do not have information on.

I control for religious denomination (Anglican, Catholic, other Christian, with not religious as reference category), region (East Midlands, East of England, North West England, South East England, South West England, West Midlands, Yorkshire and the Humber, Wales, Scotland, with North East England as the reference category) and year of observation by including year fixed effects. This is mainly to control for important factors that determine the vote that could potentially be correlated with income. I am not able to control for everything that correlates with income, and the interpretation of the coefficients should therefore be wide. The impact of income on voting in my set-up may be interpreted as how well individual voting fits into the foundations of the median voter model - namely to what degree individuals vote along the private economic dimension

[6] Voting preferences are correlated to the stated preferences for redistribution. Around 30% of the Conservative and around 70% of the Labour voters answered agree or strongly agree to the statement "Government should redistribute income from better-off" (see descriptives table 1).

(to what extent demand for redistribution/public goods is correlated with the private economic interests of individuals).

Data

I use the National Child Development Study (NCDS) and the British Cohort Study (BCS) which are detailed survey data on two British cohorts born in 1958 and 1970. It is the only data to my knowledge that has information on political voting behavior, household income and individual income over the life-cycle[7]. There is lot of potential for studying political attitudes in relation to childhood and current family environment. The BCS data are not as representative of the life-cycle with information on both incomes and voting behavior only in 1996 and 2000 when they were 26 and 30 years old. The BCS study, we will therefore mostly use for robustness.

The British political system is well-suited for an analysis of economic voting. The two biggest parties are Labour and the Conservatives, and the parties differ from each other on the socioeconomic dimension (Lijphart 1999). The government is formed by one party, which may have a minority in Parliament. Labour and the Conservatives have long been the only two "real" alternatives [8], as representation is not proportional, but decided by the winners of the constituencies. For robustness, we do the estimations without those who voted Liberal Democrats and the results are largely unchanged.

I study the impact of partners income relative to own income, and therefore I only use individuals who have a partner in my sample. I have not used the respondents who are students, unemployed or sick/disabled because I do not observe their income, and setting their income to zero would probably not be very representative. The men and women in my sample are not married to eachother. The information on the partner is given by the individual.

Housewives' income are set equal to zero. I measure the impact of income, not hourly wage, and the effect will both be a wage and a labour supply effect. I control for the individual or the partner being a housewife with a dummy. This is mostly important because it captures a clear non-linear effect of income; those who are housewives are more conservative on average, even though income is zero. One reason for this may be a smaller demand for public services as they have chosen to do the work themselves, like taking care of children and elderly.

Surveys have the problem of measurement error, and the most important measurement error in this context is the error in income. In the literature on social mobility, Solon (1999)

[7]The data have not been used often for political economy analysis, to my knowledge only three times. These are studying the effect of growing up with a lone parent (Fluori 2004), the effect of cognitive ability and personality on voter turnout (Denny and Doyle 2008) and the attitudes of the underclass (Buckingham 1999).

[8]This is true at least in my period of study - in the 2010 election, the Liberal Democrats won 57 seats in the Parliament, with the result that they formed a coalition government with the Conservatives

found that transitory income is closer to permanent income in the late thirties, beginning of the forties. An average measure over several years also gives lower measurement error. I therefore constructed a measure of mean income based on the observations when the respondents are 33 and 41 and compare the baseline results to the results using this measure.

I use the years 1981, 1991, 2000 and 2008 for the NCDS sample and the years 1996 and 2000 for the BCS sample. One source of measurement error is the fact that the last election was not immediately preceding the questioning and may therefore be colored by what they wish they voted in retrospect. The correlation between what the individual said he/she voted at the last election and what they would vote tomorrow is 0.63, not too high. Another source of bias is that income measured at the time of the survey may also be different from the income the respondent had at the time of the election. Measurement error gives a potential downward bias to my results.

The descriptive statistics in table 1 (and tables 8, 9 and 10 in the appendix) show that those who voted Conservative have on average higher own income and higher spouse income. The share of women being housewives are however quite equal in both parties, and also the share of women working part-time

Results

Income

Income has a large impact on voting behavior in all specifications in table 2. We see that for men, those who have 1% higher household income, has a 0.079 percentage point higher probability of voting Conservative. For women, the probability is 0.088 percentage point higher.

When we investigate the separate effects of own and spouse income, we see that women vote more according to their husband's income than their own, while the spouse income is insignificant for men. The association between voting and own income is four times as large for men than for women and without the inclusion of partner income, men would seem artificially more prone to egotropic voting than women.

In model 3, we investigate whether it is the maximum income of the household that is important - and we find support for this, although there is a negative significant effect of the lowest household income for men. From these estimations, we may conclude that household income is at least a better predictor of voting behavior than own income - and this is especially true for women. The political preferences of both men and women seem to follow the "representative income" of the family - the maximum income. It's clear that which spouse you have, and the spouse's income, is important for your voting behavior.

Table 1: Descriptives for Conservative and Labour voters in 1991

	Conservative		Labour		All	
	Men	Women	Men	Women	Men	Women
Net income	1518.2	510.8	990.5	420.5	1297.0	426.8
Partner income	331.7	1405.9	311.0	1162.6	326.9	1277.3
Household income	1615.9	1565.4	1165.4	1250.2	1442.4	1352.4
Wealth, own house	0.89	0.88	0.74	0.68	0.80	0.78
Married	0.74	0.78	0.67	0.69	0.69	0.72
Divorced/separated	0.089	0.11	0.090	0.13	0.096	0.13
Has children	0.64	0.73	0.62	0.80	0.61	0.75
Housewife	0.0033	0.28	0.0066	0.26	0.0039	0.27
Partner housewife	0.35	0.0012	0.32	0.0066	0.34	0.0036
Part-time	0.0050	0.27	0.014	0.31	0.0077	0.29
Partner part-time	0.28	0.0076	0.31	0.013	0.29	0.0098
Gov should redistribute	0.30	0.29	0.74	0.69	0.51	0.49
Religion						
Anglican	0.31	0.45	0.20	0.29	0.24	0.36
Catholic	0.074	0.089	0.11	0.15	0.083	0.11
Other Christian	0.060	0.10	0.074	0.10	0.085	0.12
Not religious	0.55	0.35	0.61	0.44	0.57	0.40
Geographical area						
East Midlands	0.090	0.073	0.085	0.060	0.081	0.064
East of England	0.045	0.044	0.026	0.035	0.034	0.040
North East England	0.035	0.042	0.11	0.096	0.064	0.061
North West England	0.088	0.090	0.13	0.14	0.10	0.11
South East England	0.37	0.38	0.21	0.21	0.30	0.31
South West England	0.10	0.10	0.047	0.053	0.087	0.090
West Midlands	0.11	0.097	0.095	0.080	0.095	0.087
Yorkshire and the Humber	0.071	0.072	0.12	0.11	0.092	0.091
Wales	0.042	0.044	0.066	0.075	0.055	0.055
Scotland	0.044	0.057	0.11	0.14	0.085	0.093
N	1809	2011	1524	1528	5605	5802

”Gov should redistribute” is the share answering agree or strongly agree to the statement ”Government should redistribute income from better-off”.

Table 2: The probability of voting Conservative - effect of different specifications of income, men and women

	Men	Women	Men	Women	Men	Women
Household income	0.079***	0.088***				
	(0.014)	(0.012)				
Income			0.070***	0.017*		
			(0.013)	(0.0091)		
Partner income			-0.0090	0.051***		
			(0.0088)	(0.011)		
Maximum inc in hh					0.084***	0.070***
					(0.013)	(0.011)
Minimum inc in hh					-0.015*	0.0092
					(0.0090)	(0.0088)
Wealth, own house	0.093***	0.11***	0.097***	0.12***	0.093***	0.12***
	(0.016)	(0.015)	(0.016)	(0.015)	(0.016)	(0.015)
Housewife	-0.024	0.053***	0.45***	0.12**	-0.13	0.075
	(0.083)	(0.014)	(0.13)	(0.054)	(0.100)	(0.053)
Partner housewife	0.039***	0.054	-0.049	0.34***	-0.081	0.15
	(0.014)	(0.094)	(0.053)	(0.11)	(0.054)	(0.099)
N	7055	7615	7056	7622	7056	7622

* p<0.10, ** p<0.05, *** p<0.01
OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.

This has not been a concern in political economy, with the exception of Edlund and Pande (2002) who analyse the importance of divorce rates on political voting behavior.

There is a clear non-linearity in the association between voting and income. Female housewives are more conservative on average, although their income is zero. This may be explained by "family values" of the Conservative party. The Conservatives have traditionally wished for families to have larger responsibility for children and elderly e.g, compared to the left who are relatively more in favor of the state doing more of these tasks (like kindergartens, elderly care etc). Even though households have lower income due to the wife being a housewife, they also demand less public services because they can do some of the tasks themselves.

I also did the estimations using the measure of permanent income (appendix, table 13), and the pattern is even clearer here. Household income has the highest explanatory power for women, or the maximum income in the household. Individual or maximum income has the highest explanatory power for men. For robustness, I checked whether the results were different if I only used the income at age 41 as a measure of permanent income (because there are around 27% women who are housewives at age 33 and only 13% at age 41), but this only gave lower coefficients.

For robustness, I did the estimations only for those working full-time, and the results are similar (see appendix, table 11). An important difference is that own income is more important for full-time working women than their spouse income, but spouse income is still significant. I also did the estimations without those who voted Liberal Democrats (see appendix, table 12), because comparing Conservative voters to only Labour voters might give other results. These estimations give similar results, and the reason is probably the few observations for Liberal Democrats.

Highest and lowest income in the household

We divide the individuals into separate groups according to their relative economic position in the household; whether they earn more than half the household income, less than half or in the range 0.3-0.7 of the household income (according to our measure of permanent income: mean income over the ages 33 and 41). We see that the apparent gender difference in economic voting behavior is partly due to the gender difference in economic position. For women earning more than half the household income, only own income is significant, and of nearly the same size as for men (similar to the larger effect of own income for women who work fulltime).

For men earning less than a half, however, own income is still what matters most to him. Political preferences of men seem to be formed on the basis of own income, regardless of the wife's economic position. The sample of men earning less than their wife is however small and the conclusion on this point should not be too strong.

For robustness, I did the same with the measure of permanent income (table 14 in appendix). I find that for women own income has an even clearer explanatory power both in the group that earn more than the husband, but is important also in the group that earns in the range 0.3-0.7 of the household income.

The results are connected to the level of wages for men and women in the different groups. The income is largely the same for men in all groups; men earning less than their wife earns as much as 78% the wages of men who earn more than his wife. For women this is different. Women earning less than their husband earn on average 39% of the wages of women who earn more than their husband. The total household income in families where the wife earns most is therefore higher than the average. I did the estimations for different individual income deciles and found that the effect of individual income is largest in the middle income deciles, and in the higher income deciles. There are more women in the lower income deciles, relative to men, where income has small impact on voting behavior (having a relatively higher income in the lowest income deciles still means the individual has low income relative to the mean - and a higher relative income within this group will not mean more conservative preferences).

Table 3: The probability of voting Conservative - for men and women according to their economic position in the family

	Share hh income < 0.5		Share hh income 0.3-0.7		Share hh income > 0.5	
	Men	Women	Men	Women	Men	Women
Income	0.072***	0.0090	0.062**	0.026	0.071***	0.060**
	(0.027)	(0.011)	(0.025)	(0.017)	(0.016)	(0.029)
Partner income	-0.032	0.067***	0.010	0.059***	-0.0057	0.011
	(0.029)	(0.012)	(0.015)	(0.017)	(0.010)	(0.023)
Wealth, own house	0.042	0.11***	0.061**	0.073***	0.10***	0.12**
	(0.060)	(0.018)	(0.029)	(0.025)	(0.019)	(0.053)
Housewife	0.35*	0.080	0.42*	0.14	0.75***	0.39**
	(0.20)	(0.064)	(0.22)	(0.100)	(0.21)	(0.19)
Partner housewife	-0.19	0.66**	0.047	0.35*	-0.029	0.019
	(0.20)	(0.29)	(0.099)	(0.19)	(0.062)	(0.18)
N	562	6135	2525	3200	5789	752

* p<0.10, ** p<0.05, *** p<0.01
OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.

Over the life-cycle

In table 4 I present results from the estimation of the impact of income in every year of observation, to see if income has a larger impact in some years than others. I find that income has the largest impact for men when they are 33 and 41, for women when they are 23, but also 33 and 41. We also see the signs of the wife's income being important for men at the age of 23.

The larger impact in some years might in part be variations over the life-cycle in the importance of economic factors, but could also be an indication of the role of transitory vs permanent income. When we check the life-cycle variation in the importance of our measure of permanent income (mean income over the ages 33 and 41) in table 5, we find that the effect of income is very stable over the life-cycle with a quite large estimate of the impact of income (1% higher income give 0.1 percentage point higher probability of voting Conservative for men).

For women, the effect of both own and husband's permanent income is less stable over the life-cycle, indicating a larger importance of transitory income for women relative to men. For robustness, I also constructed a measure of permanent income based on all years. Using this gave the same pattern, but smaller coefficients, underlining the importance of when we measure income for the representability of the measure.

Divorce risk

In the theoretical set-up, the role of individual income for voting behavior is influenced by the probability of divorce (p in equation 4). A higher probability of divorce will give an increased impact of individual relative to spouse income. As women have lower average incomes than men, a higher risk of divorce will make women less conservative compared to men. Edlund and Pande (2002) and Edlund et al. (2005) find as mentioned that the growing political gender gap in the US and Europe over the last decades can be explained by increasing aggregate divorce rates. With panel-data structure, I have the opportunity to investigate the effect of individual divorce risk on individual voting behavior.

I include a dummy for being divorced next period to capture the effect of perceived increased divorce risk on voting behavior. The assumption is that individuals who divorce next period have some foresight of their marriage having a relatively higher probability of divorce. I also interact own income and spouse income with the divorce-dummy to see whether own income is weighted relatively more if the individual has a higher risk of divorce. The effects of divorce risk should be interpreted with caution, however, because I have not done anything to control for the selection of individuals who divorce and there might also be biases due to a small sample of individuals who divorce (137 women and 126 men, 6.5% of the sample). Nevertheless, it might give some indication of the relevance of individual divorce risk.

Table 4: The probability of voting Conservative - for each year of observation separately, men and women

	Age 23	Age 33	Age 42	Age 50
Panel A: Men				
Income	0.024	0.14***	0.066***	0.040
	(0.048)	(0.029)	(0.018)	(0.026)
Partner income	0.066	0.0037	-0.013	-0.019
	(0.043)	(0.018)	(0.013)	(0.018)
Wealth, own house	0.084***	0.090***	0.077**	0.10*
	(0.029)	(0.033)	(0.033)	(0.058)
Housewife	-0.087	0.79**	0.37**	0.50**
	(0.29)	(0.31)	(0.16)	(0.24)
Partner housewife	0.31	0.032	-0.057	-0.084
	(0.23)	(0.11)	(0.087)	(0.13)
N	895	1983	2140	1232
Panel B: Women				
Income	0.085***	0.040**	0.031**	-0.0051
	(0.033)	(0.017)	(0.013)	(0.023)
Partner income	0.076*	0.078***	0.046***	0.034*
	(0.043)	(0.021)	(0.013)	(0.018)
Wealth, own house	0.11***	0.13***	0.10***	-0.010
	(0.027)	(0.031)	(0.029)	(0.059)
Housewife	0.43**	0.26**	0.28***	0.036
	(0.17)	(0.10)	(0.089)	(0.17)
Partner housewife		0.31*	0.22*	0.60***
		(0.19)	(0.13)	(0.21)
N	1191	2113	2357	1167

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.

Table 5: The probability of voting Conservative - for each year of observation separately using the mean of the income at age 33 and 41 as a measure of permanent income

	Age 23	Age 33	Age 42	Age 50
Panel A: Men				
Income, permanent	0.085***	0.11***	0.11***	0.12***
	(0.026)	(0.025)	(0.020)	(0.031)
Partnerinc, permanent	0.0019	-0.0039	-0.0027	-0.010
	(0.0063)	(0.0064)	(0.0074)	(0.0095)
Wealth, own house	0.081***	0.10***	0.060*	0.082
	(0.028)	(0.032)	(0.033)	(0.058)
Housewife	-0.17***	0.048	0.16	0.23
	(0.058)	(0.28)	(0.12)	(0.14)
Partner housewife	-0.028	-0.0013	0.0089	0.0077
	(0.029)	(0.031)	(0.043)	(0.051)
N	895	1983	2140	1232
Panel B: Women				
Income, permanent	0.0017	0.0020	0.013*	0.020*
	(0.0073)	(0.0065)	(0.0077)	(0.012)
Partnerinc, permanent	0.024	0.073***	0.061***	0.050***
	(0.026)	(0.019)	(0.014)	(0.019)
Wealth, own house	0.13***	0.14***	0.099***	-0.025
	(0.025)	(0.031)	(0.029)	(0.058)
Housewife	-0.019	0.019	0.13***	0.13**
	(0.027)	(0.032)	(0.041)	(0.061)
Partner housewife	0	0.21	0.26*	0.45***
	(.)	(0.19)	(0.14)	(0.17)
N	1191	2113	2357	1167

* $p<0.10$, ** $p<0.05$, *** $p<0.01$
OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.

The effect of divorce is not significant when I use all years of observation, the only year divorce is significant is in 1999 when the individuals are 42 years old. This is also the first year in my sample where women are relatively less conservative than men. British women have traditionally been more conservative than British men, but less so in the 1990s compared to 1980s (Norrander and Wilcox, 2008). The modern gender gap in voting is therefore less visible in Britain, and may be a reason why my results are not significant for all years.

The results for 1999 are shown in table 6. In the two first columns, I have not interacted the income and divorce-variable, and show the average impact of divorce-risk on voting behavior. The impact is 2 percentage point lower probability of voting Conservative for women, and 1.5 percentage point lower probability for men, but the average results are not significant.

In column 3 and 4, I interact income and risk of divorce, and the results are big and significant. There are large interaction effects of income and divorce, showing that average effects of each variable can hide great heterogeneity. When I interact income and risk of divorce, we see that the effect of divorce is negative for women. With average income (ln income = 5.61), the effect is around 22 percentage points lower probability of voting Conservative. The higher is the income for women, the less negative impact has divorce (the coefficient on the interaction is positive). The same coefficient can be interpreted as an increased importance of own income with higher individual divorce-risk. The effects are in line with what we would expect from the theoretical framework.

For men, the higher divorce-risk gives a positive effect on Conservative voting. With average income (ln income = 7.25), the effect is around 12.5 percentage points higher probability of voting Conservative. The average impact of divorce on men's and women's voting behavior is consistent with the fact that men have higher average incomes relative to women. This would isolated make women relatively worse of because of a divorce, and men relatively better off. For men with higher incomes, divorce has smaller effect on conservative voting relative to men with lower incomes (the interaction is negative). This might be connected to the result that income has smaller impact on voting behavior for high-income individuals generally.

Younger cohort - the British Cohort Study-sample

The British Cohort Study (BCS) sample born in 1970 shows a very similar pattern as the National Child Development Study (NCDS) sample (table 7). Partner income is more important for women than own income. This is however not true for the fulltime-working women who vote according to their own income (see appendix table 15). When we divide according to the economic position in the family, the groups with men earning least and the women earning most are too small to get any significant results (see appendix table 16). Women who earn least in this cohort also votes according to their husbands income.

Table 6: The probability of voting Conservative - income variables interacted with a dummy for divorce next period. Year of observation is 1999

	Men	Women	Men	Women
Divorce next period	-0.015	-0.020	0.85**	-0.45***
	(0.041)	(0.038)	(0.40)	(0.17)
Ln net income	0.068***	0.036***	0.077***	0.034**
	(0.019)	(0.014)	(0.019)	(0.014)
Ln netinc*divorce			-0.10*	0.041**
			(0.053)	(0.017)
Ln partner income	-0.0095	0.039***	-0.0086	0.037***
	(0.014)	(0.014)	(0.014)	(0.014)
Ln partnerinc*divorce			-0.023	0.027
			(0.016)	(0.022)
Wealth, own house	0.066*	0.081**	0.064	0.080**
	(0.038)	(0.034)	(0.039)	(0.034)
Housewife	0.39**	0.33***	0.45***	0.33***
	(0.18)	(0.094)	(0.17)	(0.094)
Partner housewife	-0.040	0.14	-0.044	0.17
	(0.094)	(0.14)	(0.094)	(0.14)
N	1924	2098	1924	2098

* p<0.10, ** p<0.05, *** p<0.01
OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.

Table 7: BCS sample: The probability of voting Conservative - for men and women according to their economic position in the family

	Men	Women	Men	Women	Men	Women
Household income	0.042***	0.048***				
	(0.016)	(0.011)				
Income			0.030*	0.0020		
			(0.017)	(0.0095)		
Partner income			0.00080	0.039***		
			(0.011)	(0.0099)		
Maximum inc in hh					0.043***	0.045***
					(0.014)	(0.0099)
Minimum inc in hh					-0.0065	0.00097
					(0.011)	(0.0085)
Wealth, own house	0.078***	0.070***	0.081***	0.075***	0.077***	0.070***
	(0.022)	(0.017)	(0.022)	(0.017)	(0.022)	(0.017)
Housewife	0.055	0.062***	0.21	0.048	0.032	0.049
	(0.11)	(0.018)	(0.15)	(0.064)	(0.10)	(0.057)
Partner housewife	0.035*	0.13	0.019	0.30***	-0.025	0.15*
	(0.021)	(0.10)	(0.075)	(0.10)	(0.071)	(0.082)
N	3280	4186	3284	4201	3284	4201

* $p<0.10$, ** $p<0.05$, *** $p<0.01$
OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.

When we move to the group where they earn in the range 0.3-07, we see that already here, women vote according to own income. The sample is not big enough, or the cohorts many enough to say anything about a trend, though. The results from the younger are rather reassuring in that there was not anything particular about the 1958 cohort.

Conclusion

Individual income and household income has a very different impact on voting behavior for men and women. Women on average earn less than their husband, and also vote on average more according to their husbands income than their own, or the total household income (which is closer to their husbands individual income than their own). The average pattern of voting behavior is however closely related to women's economic position in the household. If the woman earns more than her husband, she votes according to her own income. The same is true if she works full-time. If the wife is economically independent/has an income that is a larger part of the household economy, she votes more independently as well.

For men, voting behavior does not vary with their economic position in the family. Men vote according to their own income even though they earn less than their wife, and there is no significant impact of the wife's income in any of the estimated specifications.

My results show that the household is important for the individual voting behavior, especially for women. Including the spouse in political economy models may give interesting implications and dynamics in the demand for redistribution in a society. There are major societal changes in female labour force participation and divorce rates around the world, and the classic models of voting behavior will not capture fully the effect of these changes on political preferences in the society without including the household.

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Table 8: Descriptives for Conservative and Labour voters in 1981

	Conservative		Labour		All	
	Men	Women	Men	Women	Men	Women
Net income	365.6	219.8	356.2	173.6	359.1	186.0
Partner income	177.0	437.1	138.0	398.1	145.7	409.1
Household income	423.9	444.1	406.3	414.3	413.9	422.4
Wealth, own house	0.26	0.44	0.22	0.35	0.22	0.36
Married	0.30	0.52	0.37	0.58	0.35	0.54
Divorced/separated	0.014	0.031	0.022	0.049	0.023	0.049
Has children	0.097	0.22	0.20	0.40	0.18	0.33
Housewife	0	0.17	0.00054	0.27	0.00064	0.24
Partner housewife	0.092	0	0.14	0	0.14	0
Part-time	0.0053	0.038	0.0038	0.062	0.0061	0.055
Partner part-time	0.024	0.00062	0.036	0.0023	0.034	0.0019
Religion						
Anglican	0.37	0.49	0.25	0.36	0.28	0.40
Catholic	0.094	0.10	0.13	0.16	0.099	0.12
Other Christian	0.10	0.15	0.10	0.14	0.11	0.14
Not religious	0.42	0.26	0.49	0.33	0.49	0.32
Geographical area						
East Midlands	0.083	0.072	0.074	0.060	0.072	0.068
East of England	0.033	0.039	0.029	0.031	0.033	0.034
North East England	0.044	0.043	0.088	0.088	0.062	0.064
North West England	0.11	0.10	0.12	0.14	0.11	0.12
South East England	0.38	0.38	0.26	0.24	0.31	0.31
South West England	0.088	0.097	0.044	0.049	0.077	0.078
West Midlands	0.097	0.098	0.11	0.091	0.097	0.094
Yorkshire and the Humber	0.074	0.070	0.10	0.10	0.090	0.086
Wales	0.045	0.036	0.062	0.073	0.054	0.052
Scotland	0.046	0.059	0.12	0.12	0.096	0.10
N	1505	1625	1851	1725	6267	6270

Table 9: Descriptives for Conservative and Labour voters in 1999

	Conservative		Labour		All	
	Men	Women	Men	Women	Men	Women
Net income	2113.7	844.9	1914.0	703.7	1950.6	703.7
Partner income	746.1	2224.3	748.7	1682.4	699.0	1682.4
Household income	2410.8	2496.6	2321.0	1915.5	2270.8	1915.5
Wealth, own house	0.90	0.92	0.81	0.78	0.82	0.78
Married	0.78	0.78	0.70	0.70	0.71	0.70
Divorced/separated	0.072	0.081	0.098	0.12	0.093	0.12
Has children	0.73	0.78	0.72	0.82	0.70	0.82
Housewife	0.0050	0.14	0.0094	0.11	0.0080	0.11
Partner housewife	0.19	0.0047	0.16	0.0094	0.18	0.0094
Part-time	0.0067	0.30	0.015	0.33	0.015	0.33
Partner part-time	0.36	0.0056	0.37	0.019	0.35	0.019
Gov should redistribute	0.22	0.24	0.57	0.54	0.45	0.54
Religion						
Anglican	0.52	0.55	0.41	0.42	0.43	0.42
Catholic	0.090	0.11	0.15	0.16	0.12	0.16
Other Christian	0.23	0.25	0.21	0.25	0.23	0.25
Not religious	0.14	0.078	0.21	0.15	0.20	0.15
Geographical area						
East Midlands	0.086	0.071	0.080	0.074	0.080	0.074
East of England	0.050	0.049	0.038	0.038	0.040	0.038
North East England	0.030	0.040	0.087	0.078	0.062	0.078
North West England	0.097	0.087	0.12	0.12	0.11	0.12
South East England	0.37	0.38	0.26	0.26	0.29	0.26
South West England	0.11	0.10	0.067	0.073	0.094	0.073
West Midlands	0.11	0.095	0.092	0.094	0.093	0.094
Yorkshire and the Humber	0.064	0.095	0.099	0.088	0.087	0.088
Wales	0.043	0.031	0.062	0.070	0.056	0.070
Scotland	0.044	0.050	0.094	0.11	0.094	0.11
N	1195	1244	2224	2387	5626	2387

Table 10: Descriptives for Conservative and Labour voters in 2008

	Conservative		Labour		All	
	Men	Women	Men	Women	Men	Women
Net income	2710.0	1127.5	2358.0	1236.1	2333.0	1123.2
Partner income	1041.6	2622.3	1062.4	2159.3	1024.0	2250.8
Household income	3084.8	2694.9	2905.3	2534.1	2781.3	2447.1
Wealth, own house	0.91	0.91	0.85	0.84	0.84	0.84
Married	0.78	0.75	0.71	0.68	0.69	0.68
Divorced/separated	0.13	0.16	0.15	0.19	0.17	0.20
Has children	0.75	0.79	0.72	0.81	0.72	0.81
Housewife	0.0075	0.11	0.012	0.081	0.0089	0.095
Partner housewife	0.14	0.0099	0.12	0.0071	0.13	0.0080
Part-time	0.018	0.28	0.016	0.26	0.021	0.28
Partner part-time	0.31	0.026	0.28	0.044	0.30	0.029
Religion						
Anglican	0.59	0.56	0.43	0.45	0.48	0.48
Catholic	0.11	0.11	0.16	0.18	0.14	0.15
Other Christian	0.081	0.10	0.10	0.12	0.11	0.12
Not religious	0.19	0.20	0.29	0.22	0.26	0.23
Geographical area						
East Midlands	0.084	0.081	0.095	0.074	0.084	0.069
East of England	0.052	0.048	0.043	0.032	0.043	0.040
North East England	0.032	0.039	0.091	0.080	0.060	0.060
North West England	0.075	0.079	0.12	0.13	0.10	0.11
South East England	0.38	0.40	0.24	0.26	0.29	0.30
South West England	0.12	0.11	0.069	0.075	0.095	0.098
West Midlands	0.11	0.095	0.093	0.091	0.093	0.091
Yorkshire and the Humber	0.072	0.074	0.093	0.089	0.087	0.088
Wales	0.041	0.031	0.059	0.060	0.056	0.054
Scotland	0.035	0.043	0.10	0.11	0.094	0.096
N	1205	1210	1426	1445	4822	4968

Table 11: The probability of voting Conservative - sample is fulltime-workers

	Men	Women	Men	Women	Men	Women
Household income	0.071***	0.086***				
	(0.013)	(0.021)				
Income			0.081***	0.037**		
			(0.015)	(0.018)		
Partner income			-0.000085	0.024**		
			(0.0024)	(0.011)		
Maximum inc in hh					0.079***	0.082***
					(0.016)	(0.019)
Minimum inc in hh					-0.00090	0.0037
					(0.0024)	(0.011)
Wealth, own house	0.090***	0.068***	0.089***	0.072***	0.090***	0.067***
	(0.016)	(0.025)	(0.018)	(0.025)	(0.018)	(0.025)
N	7058	3374	4919	3374	4919	3374

* p<0.10, ** p<0.05, *** p<0.01
OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.

Table 12: The probability of voting Conservative - sample is without individuals voting Liberal Democrats

	Men	Women	Men	Women	Men	Women
Household income	0.100***	0.095***				
	(0.016)	(0.014)				
Income			0.080***	0.0045		
			(0.015)	(0.0099)		
Partner income			0.0034	0.062***		
			(0.0095)	(0.013)		
Maximum inc in hh					0.099***	0.079***
					(0.015)	(0.012)
Minimum inc in hh					-0.0047	0.0012
					(0.0097)	(0.0097)
Wealth, own house	0.12***	0.14***	0.12***	0.15***	0.12***	0.15***
	(0.017)	(0.016)	(0.017)	(0.016)	(0.017)	(0.016)
Housewife	0.0045	0.064***	0.52***	0.051	-0.044	0.035
	(0.098)	(0.014)	(0.14)	(0.059)	(0.11)	(0.058)
Partner housewife	0.050***	0.034	0.030	0.39***	-0.017	0.094
	(0.015)	(0.10)	(0.057)	(0.13)	(0.058)	(0.11)
N	6118	6481	6119	6487	6119	6487

* p<0.10, ** p<0.05, *** p<0.01
OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.

Table 13: The probability of voting Conservative - using measure of permanent income

	Men	Women	Men	Women	Men	Women
Hh income, perm	0.069***	0.073***				
	(0.014)	(0.012)				
Income, permanent			0.11***	0.0027		
			(0.016)	(0.0040)		
Partner inc, permanent			-0.0035	0.061***		
			(0.0038)	(0.011)		
Max income, perm					0.11***	0.082***
					(0.018)	(0.014)
Min income, perm					-0.0044	0.00090
					(0.0038)	(0.0040)
Wealth, own house	0.097***	0.12***	0.088***	0.12***	0.089***	0.11***
	(0.017)	(0.016)	(0.018)	(0.017)	(0.019)	(0.017)
Housewife	-0.040	0.040***	0.10	0.035**	0.0079	0.032*
	(0.086)	(0.014)	(0.092)	(0.017)	(0.090)	(0.017)
Partner housewife	0.023	-0.0017	-0.0020	0.29***	-0.00091	0.085
	(0.014)	(0.089)	(0.017)	(0.11)	(0.017)	(0.091)
N	6713	7305	6250	6828	6250	6828

* p<0.10, ** p<0.05, *** p<0.01
OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.

Table 14: The probability of voting Conservative - for men and women according to their economic position in the family

	Share hh income < 0.5		Share hh income 0.3-0.7		Share hh income > 0.5	
	Men	Women	Men	Women	Men	Women
Ln income, permanent	0.028	-0.0012	0.15***	0.061**	0.13***	0.085**
	(0.032)	(0.0041)	(0.043)	(0.030)	(0.021)	(0.038)
Ln partner inc, permanent	0.014	0.096***	0.0037	0.081**	-0.0028	0.010
	(0.039)	(0.015)	(0.032)	(0.035)	(0.0040)	(0.018)
Wealth, own house	0.042	0.11***	0.042	0.063**	0.085***	0.11**
	(0.060)	(0.018)	(0.029)	(0.025)	(0.019)	(0.053)
Housewife	-0.12	0.025	-0.017	-0.0046	0.23	0.062
	(0.082)	(0.017)	(0.12)	(0.029)	(0.16)	(0.083)
Partner housewife	0.033	0.21	-0.016	-0.071	-0.0044	0.0029
	(0.074)	(0.27)	(0.030)	(0.15)	(0.018)	(0.12)
N	562	6058	2525	3200	5666	749

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.

Table 15: BCS sample: The probability of voting Conservative - sample is fulltime-workers

	Men	Women	Men	Women	Men	Women
Household income	0.033**	0.052**				
	(0.015)	(0.024)				
Income			0.026	0.035**		
			(0.020)	(0.015)		
Partner income			-0.0022	0.0013		
			(0.0030)	(0.013)		
Maximum inc in hh					0.043**	0.048**
					(0.019)	(0.023)
Minimum inc in hh					-0.0031	-0.0011
					(0.0031)	(0.013)
Wealth, own house	0.076***	0.045	0.075***	0.046*	0.071***	0.046*
	(0.022)	(0.028)	(0.026)	(0.028)	(0.026)	(0.028)
N	3280	1820	2369	1820	2369	1820

* p<0.10, ** p<0.05, *** p<0.01
OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.

Table 16: BCS sample. The probability of voting Conservative - for men and women according to their economic position in the family

	Share hh income < 0.5		Share hh income 0.3-0.7		Share hh income > 0.5	
	Men	Women	Men	Women	Men	Women
Ln income	-0.053**	-0.0027	0.088**	0.044*	0.063***	0.025
	(0.025)	(0.011)	(0.035)	(0.025)	(0.018)	(0.030)
Ln partner income	-0.0045	0.053***	-0.049	0.0092	0.0034	0.0086
	(0.036)	(0.012)	(0.031)	(0.028)	(0.013)	(0.021)
Wealth, own house	0.0069	0.071***	0.061*	0.049*	0.084***	0.076*
	(0.064)	(0.019)	(0.031)	(0.025)	(0.023)	(0.038)
Housewife	-0.34*	0.024				
	(0.20)	(0.074)				
Partner housewife					0.038	0.15
					(0.085)	(0.17)
N	444	3435	1674	2144	2776	681

* p<0.10, ** p<0.05, *** p<0.01
OLS estimations with individual-clustered standard errors. Dependent variable is a dummy=1 if individual voted Conservative at the last general election. All incomes are measured in logs. Control variables are three religion dummies, nine regional dummies and year fixed-effects.